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PLATE XI.

THE PLEIADES NEBULA AND TRAIL OF ASTEROID No. 203 POMPEJA.

From a Photograph by H. C. Wilson at Goodsell Observatory Jan. 30, 1894. Exposure 4 hours.

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General Astronomy.

RESULTS OF AN INVESTIGATION OF THE ABERRATION AND ATMOSPHERIC REFRACTION OF LIGHT, MADE WITH A MODIFIED FORM OF THE LOEWY PRISM APPARATUS.*

GEORGE C. COMSTOCK.

In December, 1888, the committee having charge of the Watson Fund of the National Academy of Sciences placed at my disposal the sum of eight hundred dollars in aid of the investigations which are set forth in this paper, and I desire here to express my sincere thanks for this aid, without which the prosecution of the research would have been seriously impeded.

The observations which furnish the data to be discussed consist of measurements of the angular distance separating zodiacal stars which are very approximately 120° apart in the heavens, and the apparatus employed for this purpose was a six-inch equatorial telescope by A. Clark & Sons, provided with a system of mirrors so placed in front of its objective that images of the stars whose mutual distance was to be measured were simultaneously produced in the focal plane of the objective at a distance from each other sufficiently small to admit of accurate measurement with a filar micrometer. It has been shown by M. Loewy in the *Comptes Rendus* that the angular distance between the stars will be equal to this distance measured with the micrometer plus twice the angle between the reflecting surfaces. Since this angle does not admit of very precise determination the observing program proposed by M. Loewy is a purely differential one in which the angle between the mirrors is eliminated, but it is evident that if instead of two mirrors three or any greater number be employed, and be so placed as to make approximately equal angles, one with another, the mean value of the angles formed by the several pairs of mirrors will be determined by purely geometrical considerations, *e. g.*, in the case of three mirrors the angle will be 60°, and if each pair of mirrors be employed in the determination of the microme-

* Read at the meeting of the National Academy of Sciences, April 17, 1894.

ter distance the absolute angular distance between the stars may be obtained independently of the adjustment of the mirrors.

Practically no other number of mirrors than three can be employed, and it is the adoption of this combination that has limited my observing program to pairs of stars 120° apart. The essential difference between the method of M. Loewy and that here adopted is the elimination of the angle between the reflecting surfaces by means of the geometrical condition above considered, instead of by taking the difference between measurements of different pairs of stars made with only two mirrors. A consequence of the modification is that I have been able to determine absolute distances between the stars and to utilize the same observations for a determination of both aberration and refraction. The observing program has been so arranged that the aberration results are nearly free from the effect of undetermined error in the refraction tables and the concluded corrections to the tabular refractions are entirely independent of the aberration constant.

In my first design for an instrument the mirrors were the silvered faces of an equiangular glass prism mounted in front of the objective, with its axis at right angles to the line of sight of the telescope. Such a prism was constructed for me by Mr. Brashear and was used for a short time, but the effect of temperature changes in distorting the surfaces was so great that I was compelled to abandon it as wholly unserviceable and to substitute for the prism three plane mirrors of rectangular cross-sections which were so mounted in a metallic reel that each reflecting surface passed through the axis of the reel. Theory indicates that when this condition is satisfied any small distortion of the reflecting surfaces will have no effect upon the measured distances between the stars. Nevertheless, in order that the distortions might be made as small as possible I had silvered each of the eighteen surfaces possessed by the three pieces of glass, although only one surface on each piece was polished. By direct experiment in artificially producing effects similar to those which would result from large distortion of the mirrors I have ascertained that such magnified distortion does not sensibly affect the measurements made with the apparatus. It should be added that an opaque screen was placed between the mirrors and the objective and along a diameter of this screen three circular apertures were made each two inches in diameter. The screen was so placed that one of these apertures was nicely centered below each mirror, thus in effect resolving the six-inch objective of the telescope into three two-inch objectives each of which corresponds to a mirror. The purpose of

these screens was primarily to insure that the axes of the several pencils of light transmitted from the mirrors to the objective should all fall upon a particular diameter of the objective indicated by theory, and secondarily to obtain round images of the stars. The optical qualities of the combined mirror and two-inch objective may be inferred from my having repeatedly seen the components of α Piscium, $s = 3''$, nicely separated.

The theory of the errors of the apparatus indicates that the effect of mal-adjustment of the mirrors may be represented in terms of four instrumental constants whose values have been determined from time to time during the progress of the observations. No one of these constants has ever had at the time of observation a value so great as five minutes of arc, and since only their squares and products affect the measured distances it is apparent that the instrumental corrections to be applied are extremely minute and are sufficiently determined by even rough values of the constants. Save at the beginning of the work, when the theory was imperfectly understood, these corrections never amount to so much as $0''.1$.

The mode of observing with the apparatus is substantially as follows: The telescope is directed to the middle point of the arc joining a pair of stars about fifteen minutes before the time at which the stars attain equal altitudes, and the reel containing the mirrors is rotated into such a position that images of the stars are produced in the field of view by two of the mirrors, *e. g.*, mirror *A* produces an image of the east star, and mirror *B* an image of the west star. The distance between the images of the stars having been measured with the micrometer, the reel is rotated so as to produce successively the following positions of the mirrors, the mirror turned toward the east being always given first *AB*, *BC*, *CA*, *AC*, *CB*, *BA*. The distance between the images of the stars is measured in each of these positions, care being taken to so arrange the pointings as to eliminate the micrometer zero and to produce a symmetrical grouping of the observations about the instant at which the stars were at equal altitudes. The result of such a series of from six to eighteen micrometer settings is called an observation of the apparent distances, Δ, between the stars composing a pair.

If the refraction and aberration are known this apparent distance may be transformed into a mean distance which save for the effect of parallax and proper motion of the stars should be the same at all times. Any error in the assumed mean value of the refraction will make the absolute value of this mean distance

in error, while a periodic variation of the refraction or an error in the adopted aberration constant will introduce periodic errors into the mean distance. The investigation of the aberration and of periodic variations in the refraction must therefore be made together, and the investigation of the mean amount of the refraction may conveniently be separated from them.

For the first of these investigations each observation furnishes an equation of the form,

$$\Delta_0 + f \,.\, x + A\alpha + B\beta = \Delta$$

where

Δ_0 = the mean angular distance between the stars.
Δ = the observed angular distance between the stars.
x = the correction to the adopted observation constant.
α, β = coefficients of an annual variation of the refraction.

The numerical coefficients f, A and B have the following values:

$$f = -2 \cos \beta \sin (\lambda - \odot).$$

$$100 A = -R_0 \sin (360^\circ \,.\, \tau) \qquad 100 B = -R_0 \cos (360^\circ \,.\, \tau)$$

where

λ, β = coördinates of the middle point of the arc Δ.
τ = the fraction of the year that has elapsed at any date.
R_0 = the mean effect of refraction upon Δ at the time τ.

R_0 varies from winter to summer on account of the change in the mean temperature of the air.

The material available for a discussion of the aberration consists of 755 observations of 38 pairs of stars which are distributed with a rough approximation to uniformity throughout the twenty-four hours of right ascension, and nearly all of which lie within 20° of the equator. But before proceeding to the solution of the equations furnished by these observations it is necessary to consider some sources of error to which they are subject. The absolute term of the equation, Δ, may for the present be considered as the direct result of observation corrected for an assumed value of the aberration, plus a refraction correction computed from the Pulkowa tables, and plus a reduction to 1890.0 derived from the best proper motions available. These latter corrections are all small, since the observations extend over a period of only two years but the effect of refraction is large, amounting on the average to nearly 200″ for each observation. This correction therefore requires especial attention, and in its computation I have taken into account the difference in the force of gravity at

Pulkowa and Madison, and also the difference in the radii of curvature of the Earth's surface at the two places. I have further derived a theoretical expression for the effect of humidity upon the refraction, based upon laboratory determinations of the index of refraction of aqueous vapor (Mascart, Fizeau), and have applied this as a correction to the computed refractions. A comparison of the observations made in a dry, and in a humid atmosphere furnishes the following mean result:

$$\begin{array}{lll} \text{Humid} - \text{Dry} & = +\,0''.17 \pm 0''.08 & \text{Without correction} \\ \text{“} \quad\quad \text{“} & = -\,0''.03 \pm 0''.06 & \text{When corrected} \end{array}$$

thus confirming the correction. It was only after these corrections had been applied that I learned that substantially similar ones had been developed by Radau, *Annales de l'Observatoire de Paris, Memoires, 19.*

I have discussed the effect of temperature upon the apparatus employed and in this connection have determined from the observations the coefficient of expansion of dry air under constant pressure. The resulting value

$$m = 0.003674 \pm 0.000008$$

differs from Regnault's laboratory determination by less than half its own probable error, but is appreciably smaller than the value adopted in the construction of the Pulkowa tables. I have therefore united with the temperature correction of the apparatus, commonly called the temperature coefficient of the micrometer screw, a correction to reduce the computed values of the refraction to what they would have been had the γ factor of the tables been constructed from Regnault's value of m. The combined corrections for temperature and humidity in no case exceed $0''.4$.

Of the 755 available observations about one-fourth part were made by Mr. A. S. Flint, the remainder by myself, and before combining these different results it seemed necessary to determine the amount of the systematic difference between the observers. The results of a comparison made for this purpose are contained in the following table whose argument, d, is the apparent distance in minutes of arc between the images of stars seen simultaneously in the field of view of the telescope. A negative value of d indicates that the image of the western star was on the eastern side of the field.

d	No of Pairs.	Weight.	$C-F$	$\Delta-F$
$'$			$''$	$''$
− 8.4	5	11	− 0.67	+ 0.55
− 4.3	3	12	− .43	− .10
− 3.3	4	14	− .60	− .41
− 1.6	3	14	− .21	− .16
− 0.3	3	11	+ .33	+ .33
+ 1.6	2	15	+ .42	+ .37
+ 2.4	3	14	+ .26	+ .16
+ 4.1	3	13	+ .47	+ .18
+ 4.7	2	12	+ .13	− .26
+ 5.8	4	10	+ .59	+ .01

It is evident from the column C—F that a considerable systematic error affects the work of one or both observers, and since it appears in the sequal that this error is mainly in my own observing, I have added to the table the column Δ—F derived from C—F by applying to the latter my personal errors. The sequence of signs in this column does not indicate any appreciable systematic error affecting the observations made by Mr. Flint.

To determine the nature of the errors indicated by the preceding table I have compared my own observations with the computed values of Δ derived for each pair of stars from the right ascensions and declinations of the stars, using a set of places derived from all the modern catalogues of precision supplemented by an extensive series of determinations of right ascension made for this purpose by Mr. Flint with the meridian circle of the Washburn Observatory. The results of this comparison are contained in the following table where Δ denotes such a computed distance. The quantities *d* and C have the same meaning as in the preceding table:

d	No. of Pairs.	Weight.	$\Delta-C$	$\Delta'-C'$
$'$			$''$	$''$
− 7.0	9	10	+ 0.56	−0.52
− 2.8	7	10	− 0.40	− .57
− 0.3	7	10	− 0.53	− .53
+ 2.6	6	10	− 0.77	− .62
+ 5.2	7	11	− 1.08	− .49

The sequence shown by the values of Δ—C could be in great part removed by altering the adopted value of a revolution of the micrometer screw, and the column Δ'—C′ exhibits the results which would be obtained were this value diminished by 0″.07. But against the introduction of such a correction there is to be urged that it offers no explanation of the differences C — F, and that the adopted value of a revolution of the screw depends upon the accordant results of two entirely independent methods of determination, *i. e.*,

One revolution $= 27''.412 \pm 0''.004$ from transits of stars.
$= 27\ .412 \pm 0\ .004$ from the meridian circle

which render the presence of so gross an error as $0''.07$ exceedingly improbable. Neither can the differences $\Delta - C$ be due to a progressive error in the micrometer screw which causes the value of a revolution to vary with the distance measured, since these errors have been investigated both by means of the meridian circle and by the measuring engine of the Transit of Venus Commission at Washington. Both of these methods furnish very small and accordant results which have been duly applied to the observations.

A possible explanation of the differences may be found in the supposition of a fixed habit of observing on the part of C by which the micrometer threads are not placed upon the centers of the star images but are set a little farther apart than the images. Such a personal error may easily arise from the necessity experienced by the observer of turning his attention rapidly from one star to the other in estimating the coincidence of the micrometer threads with the images. Such a habit of observing would give too great values of Δ with positive d's, and too small values with negative d's; but it appears to me probable that with increasing distances the errors would increase more rapidly than d, and I have therefore chosen, somewhat arbitrarily, to represent these errors as functions of the square of d. From a least square solution of the data furnished by thirty-eight pairs of stars I find as the definitive expression for this correction when d is expressed in minutes of arc

$$\varphi(d) = \pm\ bd^2 \qquad b = 0''.0173 \pm 0''.0017$$

The legitimacy of this correction should not be considered as depending upon the hypothesis above advanced with regard to its origin. It is an empirical term whose justification is that it brings the observations by C into agreement with the computed distances between the stars, and is presumably of subjective origin because the observations by F require no such correction. I have adopted it and applied it to all of my own observations.

All of the corrections above considered having been duly applied the observations of each pair of stars were treated by the method of least squares and a set of normal equations formed for each pair. These several groups of normal equations contain three unknown quantities which are common to all of them, *viz.*, the correction, x, to the adopted constant of aberration and the two coefficients α and β of a supposed annual term in the refraction. They also contain a term which is peculiar to each pair of stars, the correction to the assumed distance between the stars.

This term being made the first in the elimination the summation of the values of $[bb\,.\,1], [bc\,.\,1]$, * * * $[dn\,.\,1]$, etc., resulting from the several sets of normal equations furnished a final system of normal equations containing only the unknown quantities x, α and β.

I have made three such solutions of the data, differing from each other in the following respects:

A. This solution adopts the refractions of the Pulkowa Tables and assumes that the correction above represented by $\varphi(d)$ is illusory, and that the observations by *C* require no empirical correction.

B. This solution adopts $\varphi(d)$ as a real correction and applies to the Pulkowa refractions the corrections above discussed.

C. The same as *B* except that in forming the final normal equations every pair of stars for which $[bb\,.\,1]$ was less than 25 was rejected. This is equivalent to rejecting those pairs of stars which were observed under unfavorable conditions for a determination of aberration, the coefficient presenting a very small range of values. The pairs of stars thus rejected were introduced into the observing list for the determination of the absolute amount of there fraction and the conditions determining their choice were such that in most cases they present large values of d; the observations are of a slightly inferior degree of precision, and are much more affected by any uncertainty in the empirical term $\varphi(d)$.

Since I prefer the results of solution C, I give the normal equations resulting from it alone, but I give the resulting values of the unknown quantities from each solution.

NORMAL EQUATIONS FOR THE ABERRATION.

$$
\begin{aligned}
+\,952.28x - 133.81\alpha - 119.99\beta - 3.61'' &= 0\\
-\,133.81x + 844.92\alpha - 414.62\beta + 45.09 &= 0\\
-\,119.99x - 414.62\alpha + 571.59\beta - 20.31 &= 0
\end{aligned}
$$

The values of the unknown quantities furnished by the several solutions, together with their probable errors and the probable error of a single observation are as follows:

	Const. Ab	α	β	r_1
	" "	" "	" "	"
Solution A	20.499 ± 0.010	− 0.012 ± 0.013	+ 0.075 ± 0.016	± 0.32
B	.440 ± .010	− .045 ± .013	− .010 ± .016	± .32
C	.443 ± .010	− .058 ± .013	− .008 ± .016	± .30

The difference between the values of the aberration furnished by solutions *A* and *C* is due almost entirely to the introduction of the empirical correction $\varphi(d)$ and does not arise from the correc-

tions applied to the refraction. It may be noted as a singular coincidence that the effect of $\varphi(d)$ is to transform a value of the aberration in close agreement with that of Nyrén into one which agrees even more closely with that of Struve.

The effect of the correction to the refractions is shown in the quantities α and β whose character is completely changed by its introduction. These quantities were introduced into the equations as the representatives of an assumed annual variation of the refraction not taken into account by the tables. Their significance is most conveniently shown by transforming them into the equation

$$R = R_0\{1 - 0.00058 \sin (360^\circ \,.\, \tau + 8^\circ)\}$$

where R denotes the actual, and R_0 the tabular value of the mean refraction at a date expressed by τ, the elapsed fraction of the year. The probable error of the numerical coefficient is

$$\pm\ 0.00013.$$

It appears from this equation that the refraction in the spring is less, and in the autumn greater than its mean value by about one-twentieth of one per cent.

Since the form of the equations is such that α and β are necessarily assigned real and finite values by the solution, the very small variation in the refraction indicated by the coefficient of R_0 may be considered to have no real existence and to be produced by the unavoidable errors of observation, although the small value of the probable error does not support this view. If, nevertheless, this supposition be adopted and the values of α and β in the expression for the aberration be put equal to zero, we shall obtain for the value of the constant $20''.452 \pm 0''.010$. It appears to me better to retain the values of α and β furnished by the equations since they represent not only the effect of a variable term in the refraction but also the combined influence of all terms whose effect upon the observations varies with the seasons. Whatever physical interpretation is placed upon the quantities it is apparent that the Pulkowa tables represent the seasonal variation of the refraction with surprising accuracy.

I adopt as the definitive result of this part of the investigation

$$\text{Constant of Aberration} = 20''.443 \pm 0''.0106.$$

The probable error includes the effect of uncertainty in the value of a revolution of the micrometer screw.

Definitive values having been found for the quantities thus far considered it becomes possible to derive from the observations of

each pair of stars the angular distance between its components, and a comparison of this measured distance with that computed from the coördinates of the stars may be used to determine a correction to the absolute amount of the tabular refraction. It should be noted that since the observing program contained only equatorial stars the computed distances will depend mainly upon the right ascensions and will be but little affected by small errors in the adopted declinations.

The results of such a comparison are exhibited in the following table, in which there are united into mean values the individual results derived from all those pairs of stars the middle points of whose joining arcs fall within convenient limits of right ascension. The adopted coördinates of the stars were referred to the system of the *Berliner Jahrbuch* which is substantially the same as that of the *American Ephemeris.* I have included in the table columns showing the differences between the computed and observed distances which would have been obtained had the right ascensions of the stars been reduced to the respective systems of the *Nautical Almanac* and the *Connaissance des Temps* of the year 1883.

COMPARISON, C—O, WITH THE EPHEMERIDES.

Limits of R. A.	No. of Pairs.	B. J.; A. E.	N. A.	C. T.
h h		"	"	"
1.1... 3.2	2	+ 1.03	+ 2.13	+ 2.42
5.5... 6.4	4	+ 0.32	+ 0.80	+ 0.72
7.1... 7.5	3	+ 0.70	+ 0.82	+ 0.81
8.4... 9.3	4	+ 0.11	+ 0.26	− 0.20
9.9...11.1	4	− 0.20	− 0.16	− 0.56
12.9...13.5	2	+ 0.08	+ 0.28	− 0.22
15.0...15.6	2	+ 0.43	− 0.27	− 0.33
17.5...18.6	4	+ 0.20	− 1.08	− 0.74
18.8...19.6	3	− 0.11	− 1.11	− 0.87
20.4...21.2	3	+ 0.06	− 0.45	− 0.31
21.6...22.2	4	+ 0.48	+ 0.44	+ 0.49
22.7...23.5	3	+ 0.08	+ 0.53	+ 0.61

The differences between the values of C — O which stand on the same line of the table are due solely to the differences between the ephemerides, and the very considerable values which they attain illustrate the uncertainty affecting the computed length of any long arc in the heavens, even when derived from the coördinates of fundamental stars. The comparisons with the *Nautical Almanac* and *Connaisance des Temps* show unmistakably a periodic variation of C—O depending upon the right ascension and having a period of twenty-four hours. The *Berliner Jahrbuch* and *American Ephemeris* seem to be nearly free from error of this kind, although there is some indication of it. It does not appear

that this variation in the-values of C — O can be attributed to the observations since the method of discussion is such that very term having a period of twenty-four hours must have been eliminated through the introduction of the constants α and β.

To determine the mean value of C — O, I have made a graphical adjustment of the numbers in the third column of the table, and from the thirty-eight residuals furnished by comparing the adjusted values with the values of C — O furnished directly by the observations, I find for the probable error of a single C — O, $\pm 0''.33$, and for the probable error of a single right ascension used in the computation of the distances, $\pm 0^s.015$. According to the discussion by Dr. Auwers (*Astr. Nachr.* No. 2714) the probable error of a right ascension of the best determined *Haupsterne* of the *Berliner Jahrbuch* is, for the epoch 1891, $\pm 0^s.014$, *i. e.*, the average right ascension employed in the above discussion has approximately the same degree of precision as that of the best determined fundamental stars. It also appears that a single observation with the apparatus employed will furnish a better determination of the distance between two fundamental stars than can be obtained from the best coördinates available at the present time.

The adopted mean value of C — O is $+ 0''.36 \pm 0''.05$, indicating that the tabular refractions must be increased in order to satisfy the observations. Since this value depends upon the adopted coördinates of the stars, and may be supposed to be affected with some residual error from this source, it seems proper to show that a correction to the refraction may be derived which shall be entirely independent of the adopted right ascensions. The observing program contained six triplets of stars, each of which consisted of three stars within five degrees of the equator, each star of a triplet differing in right ascension from each of the others by approximately eight hours. The three arcs joining the stars of a triplet, taken in pairs are each very approximately 120° in length, and their sum when projected upon the equator must be exactly 360°. The refraction is to be determined from the condition that the sum of the projected arcs of each triplet, *i. e.*, the measured circumference of the heavens, shall equal 360°.

The individual values of C — O furnished by the several triplets, together with their weight are as follows:

Triplet.	A	B	C	D	E	F
C — O	$+0''.38$	$+0''.40$	$+0''.27$	$+0''.17$	$+0''.59$	$+0''.17$
Weight	5	5	3	3	4	6

The weighted mean of the several results is $+ 0''.31$, and the

close agreement of this value with that above found from the adopted right ascensions indicates that the residual error affecting the latter is well eliminated from their mean. I adopt as the definitive value of C — O, $+ 0''.34 \pm 0''.04$. Since the average value of the refraction for all the observations is $195''.0$ it appears that the refraction with which the observations were reduced must be multiplied by the factor, $1 + \frac{0.34}{195} = 1.00173$, or to $\log \alpha$, which in the Pulkowa tables is called μ, there must be added, 0.00075 ± 0.00005.

If we combine the several corrections derived for the Pulkowa refraction tables and represent their effect as a correction to the mean refraction it may be put in the form

$$\Delta \log \alpha = \Delta \mu = + 0.00075 - 0.00007{,}6b$$

where b is the mean value, in millimeters, of the aqueous vapor tension during clear weather at Pulkowa. I have no adequate data from which to derive a value of b. Its mean value for Madison is 6.6 and it seems probable that a somewhat greater value should be assigned for Pulkowa, but probably not so great as 10mm. Any value of b within these limits will furnish as a correction to the mean refractions a quantity less than $\pm$ 0.00028 which according to the data contained in *Obs. Poulk.* Vol. 5, is the probable error of the constant of the tables. The result of the investigation is therefore that the actual error of the Pulkowa mean refractions is less than the probable error assigned by the data from which they were constructed.

The comparison with Bessel's refractions (*Tab. Reg.*) is much less satisfactory. The temperature factors of this table, γ, are considerably in error and the mean refraction requires a correction represented by

$$\Delta \log \alpha = - 0.00063 - 0.00007,\, 6b$$

Assuming the mean aqueous vapor tension to be 8mm the above correction is equivalent to multiplying the refractions by the factor 0.99715, agreeing approximately with the factor 0.9959 found by Nobile from corresponding observations at Capodimonte and Cordoba, and with the factor 0.9988 adopted by Stone at the Cape of Good Hope. It is however doubtful if the defects of Bessel's table can be cured by the use of any constant multiplier.

I cannot conclude this paper without touching upon one further conclusion to which these investigations have led me. The so-called anomalous refractions which have been made to serve as the explan-

ation of so many discordances in observations of zenith distances may be greatly reduced in magnitude by a more careful determination of the temperature at the objective of the telescope and by taking into account the barometric and thermometric gradients, *i.e.* the inclination to the Earth's surface of the strata of homogeneous air. From a discussion of the observations with the Pulkowa vertical circle, which are doubtless the most accurate zenith distances ever measured, Gylden finds as the probable accidental error of a computed *refraction*, R, $\pm$ 0.0015 R, which corresponds exactly in amount to the probable error of a single *observation* in the present series. Since a considerable part of the latter probable error must arise from instrumental and personal sources it appears that the attainable precision of the refractions has been underestimated and that errors chargeable to other sources have been attributed to fortuitous irregularities in the refraction. Since the effect of refraction upon the angular distance between two stars is nearly independent of their zenith distance, the effect of inclination of the strata of homogeneous air to the plane of the horizon is completely eliminated from the present series of observations. It is in large part due to this circumstance that the precision of the observations, $r = \pm$ 0''.30 at a zenith distance of 68°, exceeds what has hitherto been attained.

ON THE DIAMETERS OF CERES, PALLAS AND VESTA.

E. E. BARNARD.

Our great telescope would seem eminently suited for a micrometrical determination of the diameters of those of the minor planets that have sensible discs.

In examining the brighter asteroids I found them to be readily measureable with the large telescope, and that their diameters can be obtained with a reasonable amount of accuracy. I have, therefore, undertaken the work of measuring a few of these bodies with the 36-inch.

Various attempts have been made previously to determine their diameters micrometrically with smaller instruments. These results are very discordant.

They have also been observed photometrically, but this method, it seems to me, is one of considerable uncertainty as it rests upon the assumption that their reflective surfaces are the same as that of some particular standard with which they are compared.

Such an assumption is rather risky and the results can only be very approximate at best.

I have so far succeeded in getting direct measures of Ceres, Pallas and Vesta, and I hope to continue the work as it is one of considerable importance.

As I have said, the previous measures of these bodies are very discordant, and were evidently made with instruments too small to deal with the subject. A collection of these measures, up to 1881, can be found in the *Vade-Mecum* of Houzeau.

I will copy here these values as there given for the four brighter asteroids.

CERES (1)

1802	W. Herschel	measured	diameter	0″.127
1805	Schroeter	"	"	1 .259
1839	Galle	"	"	0 .32
1866	Knott	"	"	0 .510

PALLAS (2)

1805	Schroeter	measured	diameter	1″.626
1807	W. Herschel	"	"	0 .09
1837	Lamont	"	"	0 .26

JUNO (3).

1805	Schroeter	measured	diameter	1″.144

VESTA (4).

1816	Schroeter	measured	diameter	0″.313
1847	Mädler	"	"	0 .272
1855	Secchi	"	"	0 .428
1881	Tacchini	"	"	0 .830
1881	Millosevich	"	"	0 .597

These, I assume, are all reduced to distance unity.

In another table the diameters of these bodies are given in miles.

Following are the values derived by Argelander from considerations of their relative light.

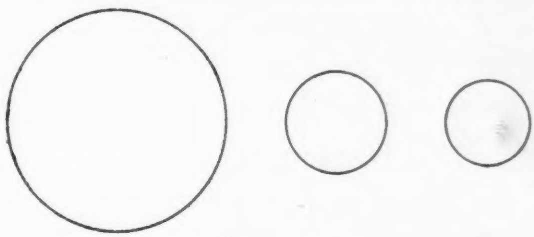
Ceres (1)	diameter,	230	miles
Pallas (2)	"	162	"
Juno (3)	"	108	"
Vesta (4)	"	275	"

Mr. E. J. Stone in 1867 determined their diameters also from a consideration of their light, based upon the measures of Ceres and Pallas by Herschel and Lamont respectively. His results are:

Ceres (1)	diameter,	196 miles
Pallas (2)	"	171 "
Juno (3)	"	124 "
Vesta (4)	"	214 "

It will be seen that in both these tables, Vesta comes out the largest of the asteroids.

In connection with the above, I have thought it might be interesting to give my own measures, so far obtained, of the diameters of Ceres, Pallas and Vesta. I have not yet secured any measures of Juno, but will as soon as it is in favorable position.



CERES. PALLAS. VESTA.

The measures have been made with the 36-inch equatorial, and uniformly with 1,000 diameters. The details of the observations are reserved for a later and more complete paper.

FILAR MICROMETER MEASURES OF THE DIAMETERS OF CERES, PALLAS AND VESTA, MADE WITH THE 36-INCH EQUATORIAL.

CERES (1).

Diameter 1″.330 ± 0″.064 at distance unity.
Diameter 599 ± 29 miles (3 nights' measures).

PALLAS (2).

Diameter 0″.605 ± 0″.026 at distance unity.
Diameter 273 ± 12 miles (4 nights' measures).

VESTA (4).

Diameter 0″.527 ± 0″.033 at distance unity.
Diameter 237 ± 15 miles (3 nights' measures).

During these measures it was readily apparent that, though Pallas and Vesta were nearer than Ceres, the latter was noticeably twice as large as either of them. It will be seen that the measures also show this, thus proving that Vesta is certainly not the largest of the asteroids, and placing Ceres at the head of the list.*

* An inspection of Juno, without measures, shows it to be about comparable with Pallas and Vesta in size.

These three are apparently round and uniform in their light. Pallas is yellowish, Vesta slightly yellowish, while Ceres is whitish. There was nothing in the appearance of their discs to suggest any specially extensive or dense atmosphere.

The diagram on the preceding page is intended to show at a glance the relative sizes of Ceres, Pallas and Vesta, from my measures.

MT. HAMILTON, 1894, March 26.

THE PERSEID RADIANT.*

W. H. S. MONCK.

Mr. Denning in his article on the Perseid Radiant in *Popular Astronomy* for February reiterates his statement that the Perseid Radiant shifts its position from night to night moving eastward (witn a slightly increasing declination) at the rate of a little over 1° each night from the 19th of July to the 17th of August. The duration he thinks may possibly be longer but the observations are rather too scanty to warrant that conclusion.

So far as I have been able to ascertain this shifting of the radiant is only established by selecting from among the meteors which come from Cassiopeia or Perseus at this period a certain number which fulfil the alleged law to which the name of Perseids is applied and rejecting as not genuine Perseids the meteors which do not fulfil the law. This will I think appear from an examination of Mr. Denning's great Catalogue (the most valuable that we at present possess) in the *Monthly Notices* of the R. A. S. for May 1890.

His initial position is at 19° + 51° on the 19th of July. He observed the radiant but once and then deduced it from 4 meteors only. He does not appear to have observed any meteors from the shifting radiant on the 20th or 21st but on the 22d and again on the 23rd he observed meteors coming from 25° + 52°. It is then lost sight of until the 27th when he finds it at 29° + 54° and on the same day he also obtained a radiant at 32° + 53°; on the 28th his determinations in different years gave 27° + 55°, 30 + 55° and (twice) 32° + 53°, after which he obtained meteors pretty nearly in accord with his shifting radiant up to the end of the maximum on August 12 if not later. But these meteors did not stand alone. On the 20th of July he obtained meteors from 21° + 57°. These might be referred to the shifting radiant if we

* Communicated by the author.

suppose an error of some degrees in the declination; but meteors from the same point were observed on July 28, July 29 and Aug. 1. Again the radiant at 32° + 53° gave meteors not only on the 27th and 28th of July but on the 30th and 31st and on the first of August, and if we identify it with a radiant at 31° + 54½° observed on the 29th of July we seem to have six days of continuous activity for this radiant without any change of position. The radiant at 21° + 57° on July 20th seems to turn up again at 20° + 58° on the 2d and 4th of August. 18° + 63 is another favorite radiant for meteors during this period. It was active on July 19, 20, 21, 23 and 24, and is no doubt identical with a radiant observed at 20° + 65° on July 22; and after some tolerably near approaches it is again found active on August 7 and 12. Even as regards the main Perseid radiant, Mr. Denning observed meteors from 43° + 58° on July 27, 28, 29 and 31 from 42° + 57° on Aug. 5, 41° + 58° on August 7 and 40° + 59° on August 20 and 21. (I have used the column "Other Nights of Observation" as well as the principal column in making out these details). All these meteors possess the same swift, streaky character.

I therefore conclude, 1st, that the shifting radiant has not been observed at every point of its supposed career, but that its position on several days has been obtained by interpolation. This is a legitimate process after the shifting has been proved but not till then. 2nd, the starting position is deduced from a very few meteors and I may make the same remark as to the final position. 3rd, several points on (or nearly on) the track of Mr. Denning's shifting radiant produce meteors both before and after this shifting radiant is supposed to pass over them—this being specially true of the principal radiant and of another radiant situated at about 32° + 53°.

In fact I believe both of these are permanent radiants. The principal radiant situated at somewhere about 44° + 57° is not only active (at least intermittently) from July 27 to Aug. 21, but before and after these dates. It turns up at intervals in Mr. Denning's Catalogue from the 14th of June to the 10th of December. The radiant at 32° + 53° does not seem to occur earlier than the end of July when its activity is at a maximum; but I find it at 32° + 50° on Aug. 14.23, at 33° + 54° on Sept. 6.9, at 31° + 52° on Sept. 21.25 and at 32° + 50° on Oct. 8.

I have confined myself in these remarks to Mr. Denning's own observations. I might perhaps have confirmed them by introducing the observations of others. But I suspect that there is a good deal of what is called "personal equation" in the observa-

tions of the majority of observers which renders their figures hardly comparable with each other. Mr. Denning is one of the oldest and most painstaking of meteoric observers and his observations may at all events be regarded as following the same scale. And so far as Mr. Denning confines himself to observation I have no quarrel with him. But the shifting of the Perseid radiant is not a fact of observation, but an inference from observations, and in my opinion an inference which the observations do not warrant. Nor is the greatest skill in making observations any protection against drawing erroneous inferences from them. All Mr. Burnham's accuracy in determining the distances and positions of a pair of binary stars does not render him infallible in his determination of the orbit; but to make the cases really parallel, we should suppose that the larger star was surrounded by a number of smaller ones which could only be glimpsed occasionally on a clear night, and that Mr. Burnham's orbit was made out by assuming the identity of a certain number of these glimpse-stars notwithstanding that small stars had more than once been glimpsed in the old places of supposed *comes* long after the latter was supposed to have moved further on in its orbit.

It is true that on the current theory a meteor radiant ought to shift night after night, and I may add that I do not see how it could continue for anything like a month. But as a matter of fact the great majority of radiants which continue for some nights do not shift. Where the radiant is not a diffused one it seems to be almost absolutely fixed, and in the case of a diffused radiant there is, as a rule, no steady shifting but rather a fluctuation of the supposed central point. The current theory must be wrong, for it does not explain the facts. When it is replaced by a theory which affords an explanation of the fact of stationary radiation (the ordinary case) the new theory will probably show whether there are exceptional circumstances which might cause a shifting, and whether these exceptional circumstances occur in the case of the Perseids or not. The facts as at present known do not seem to me to establish any shifting; but they might perhaps also be explained (1), by the shifting of a diffused radiant, or (2), by a shifting radiant passing nearly over two or more stationary radiants in its course. The latter is probably Mr. Denning's idea, but I do not think it will explain the facts unless there is a good deal of diffusion in the shifting radiant itself. I include the existence of sub-radiants under the head of diffusion.

THE CANALS OF MARS.*

J. R. HOLT.

The general opinion with regard to Schiaparelli's canals is that they represent cracks in the crust made during solidification, and in consequence, that they are older than the seas. On the other hand, some astronomers incline to the view that they are artificial; the inhabitants having rectified the rivers and dug other canals, probably with a view to irrigation. Other hypotheses which I have seen put forward are, (1) that they are furrows formed by aerolites, (2) that they have been formed by tidal erosion. A consideration which seems to me to constitute to some extent a discriminating test between these views is as follows.

If the canals are cracks which existed before the present seas, we might expect that some of the longer cracks run across the sea bottoms: generally speaking, they would be invisible while actually traversing the sea, but would reappear on islands or continental surfaces. On these, they would appear as continuations of the original cracks. It is even possible that under very favorable circumstances, the canals may actually be traced across the shallower seas. On the other hand, if they result from the rectification of the rivers, any such prolongation, if observed, must be merely accidental, and although, if they are entirely artificial, we cannot deny that the inhabitants might have *some* object in so constructing them, we cannot imagine what the object could be. Consequently, if such prolongations are observed in greater number than can fairly be ascribed to mere coincidence, the fact is so far evidence in favor of the more general view, as against the hypothesis that they are artificial.

Now, on a careful examination of Schiaparelli's map, given at p. 440 of M. Flammarion's "La Planète Mars", I have come to the conclusion that such prolongations do exist. The following cases seem unmistakable.

(1) Eumenides—Nectar, (2) Pyriphlethegon—Ambrosia, (3) Triton—Ascanius, (4) Gigas—Scamander, (5) Galaxias—Xanthus, (6) Astaboras—Nepenthes.

The following, although more doubtful, seem reasonably probable.

(1) Anubis—Alpheus, (2) Cerberus—Peneus, (3) Tartarus—Herculis Columnæ. Another consideration is that these cracks were lines of weakness in the crust, and therefore probably had consid-

* Communicated by the author.

erable influence in determining the distribution of sea and land. Thus the Mare Cimmerium seems to have some connection with the Triton; the Mare Tyrrhenium with the Typhon; the Mare Sirenum probably with the Tartarus; and Herschel II Strait, and possibly the Hadriaticum Mare, with the Hydraotes. Professor Pickering has traced some of the canals across the seas. This is also a further confirmation. Lastly, we can find many instances of canals apparently prolongations of one another, in which the intervening portion crosses the *land*. Thus, for instance, the Typhon—Jordinas.

In these cases, the most obvious view is that the intervening portion of the canal exists, but that for some reason, it is not conspicuous. I drew up a map, made by copying Schiaparelli's chart, and marked these hypothetical canals, to the number of some 20 or 30, on it. Almost every canal, shown on drawings made subsequently to Schiaparelli's chart, falls into its place on it.

All these arguments make for the view that the canals are cracks in the crust, as against the view that they are artificial.

But they make with equal force against the tidal erosion and aerolite hypotheses. In addition, there are other arguments against these views. Thus the tidal view is that the water found its way from one ocean to the other across the land. If the canals were straight parallel lines, which did not intersect, this view might be tenable; but I cannot see how the straight intersecting lines could be so formed. So with regard to the aerolite hypothesis; the idea seems to be that the rarified atmosphere of Mars would not be an effective screen. But this leaves out of consideration the slow decrease of density. If we take this into account, we find that, even if the density at the surface is only one-tenth of ours, the atmosphere is a much more effective screen against meteorites than ours.

Accepting the view that the canals are cracks in the crust, the next point is as to the mode of formation. The suggestion which first occurs to the mind is that they were formed by contraction of the crust during cooling; but it is not certain that similar cracks *could* be formed in this way. M. Flammarion mentions that M. Daubrèe tried to produce a similar appearance by covering a hollow caoutchouc globe with a layer of paraffin, and subjecting it to pressure. Nothing resembling the canals was produced, although our terrestrial mountains were fairly imitated; on the other hand, when the globe was distended by water being forced into it, similar cracks *were* produced. M. Lebour has

pointed out the resemblance of these canals to cracks in glass broken by torsion. Another consideration is, that when a molten body solidifies, at first the crust contracts more rapidly than the still molten nucleus; the crust accordingly closes in on the latter, and compresses it, so that the molten matter is forced up through the cracks; this may be what has occurred in the case of the Moon, and the great difference in aspect between the cracks in the Moon and Mars, is evidence that there is a fundamental difference between the ways in which they originated. Taking all these considerations into account, I have been led to form the following hypothesis.

A molten mass of badly-conducting material freely cooling, would soon form a thin crust, while the matter a short distance below the surface was still perfectly liquid. This formation of a thin crust is in general prevented by the disengagement of gas from the molten mass, which breaks it up at once, before it has time to get thick enough to have sufficient strength to withstand the gas.

Now I suppose that in the case of Mars, for some reason, there was a temporary lull in the emission of gas, sufficiently long to allow of the formation of a thin crust, so strong as not to be broken up by the gas when emission began again, but not strong enough to be able to compress the gas when contracting. Consequently, when it began to contract, being prevented from closing in by the expansive force of the imprisoned gas, it shrank laterally, forming long rents. It is even possible, that between the force of expansion tending outward, and gravity tending inward, portions of the crust may have been subjected to actual torsion, and some of the canals may have been formed under its influence; in general, however, my suggestion is that they are, properly speaking, rents formed by lateral shrinkage. As soon as these fissures had formed, the imprisoned gas escaped, and the ruptured crust fell in upon the nucleus; but by this time, the latter had cooled to some extent on the surface, and hence was not easily forced up through the fissures; besides, the only force tending to so drive it up would be pressure caused by gravity, resulting from the cooled fragments of the crust being somewhat denser than the still molten material; there would be no force of compression acting. Under these circumstances, the molten matter would not be forced up through the fissures, or at any rate, not completely, so as to fill them.

The resemblance of this hypothesis to M. Rateau's hypothesis of sub-continental gaseous belts, *L' Astronomie, 1893, p. 412*, will

be at once apparent. In fact, the idea occured to me eighteen monthsago, but I dismissed it as probably absurd, until the publication of M. Rateau's hypothesis.

The fundamental difference between the cases of the Earth and Mars, I take it, is as follows: In Mars a thin crust was early formed and ruptured. The gas then escaped, and the fissures served as vents for the internal volcanic energy, so that the crust has remained almost in its primitive condition ever since. On the Earth, the crust was not formed so early; in consequence, it escaped rupture as a whole, and was able to compress the gas.

The volcanic energy, not having any preformed vents, deformed the crust considerably, and formed vents for itself here and there. These, however, were insufficient to allow of a complete escape; in consequence, the surface has suffered from volcanic action to an amount contrasting considerably with Mars.

This hypothesis is of course very crude; however it may possibly suggest a better one.

A curious fact, which the above does not explain, and which should be explained by any adequate theory, is the way the canals fall into parallel groups. Thus in (1) we have Phison, Hiddekel, Oxus, Jamuna, Chrysorrhoas, Cyclops; in (2) Asutapes, Anubis, Euphrates, Gehon, Hydaspes, Ganges; in (3) Araxes, Gigas, Avernus, Cerberus. Many other groups may similarly be noted. Portions of the coast often are parallel to the canals in one of these groups. Thus the coast of the Hour-Glass Sea, from the mouth of the Asutapes to Cape Banks is parallel to group (1).

So the coast at one side of Margaritifer Sinus is a continuation of the Oxus; one side of Auroræ Sinus is parallel to Nilosyrtis, Astaboras, Typhon, Jordanis, Hydraotes-Nilus, Eumenides, Erinnys, etc. This suggests that these coast-lines are really canals. Direct evidence of a portion of the coast being a canal, is, of course, very seldom attainable. In M. Schiaparelli's obserservation of 20 June, 1890, *La Planète Mars*, p. 476, he saw the south coast of Beer Continent (Herschel II Strait) transformed into a double canal. This is to some extent confirmation of my hypothesis; *i. e.*, when the fragments of the crust, settled down on the nucleus, abrupt changes of level only occurred at the edges, that is, the canals.

Passing on from the question of origin, a new question arises as to the present state. The most usual opinion is that the canals are rivers. In favor of this theory we have (1) their dark colour, like water, (2) the fact that no other rivers have

been observed, (3) the consideration that water would certainly find its way into these ravines, (4) the way in which they termin- ate in bays on the coast. Against this theory we have (1) the way in which these canals run from ocean to ocean, (2) their nearly uniform width, (3) the consideration that if all are rivers, Mars is much more richly provided with large rivers than the Earth.

At this juncture, we may possibly be helped by considering what would take place, if the Earth were similarly seamed with ravines.

Obviously, the surface drainage would sooner or later find its way into them, and so we would have rivers formed; it is also obvious that the surface drainage would in general not be suf- ficient to fill them completely, so that we would have ravines with rivers flowing down the middle. Obviously, too, although a ravine might reach from ocean to ocean, the contained river would not do so. In fact, the one ravine might contain two rivers, flowing in opposite directions, one discharging into one ocean, the other into the other. Now, that this should be the case with the canals, it is necessary to suppose that the dry portion of each ravine, on either side of the river, is undistinguishable from water; either through the soil being of a dark colour, or through it being clothed with a dark-coloured vegetation.

I will return to this point again; for the present I will pass over the question as to *what* gives the dark colour to the soil.

Now in this hypothesis, that only the middle of each canal is water, we find a possible explanation of an observation made by Schiaparelli on Dec. 26, 1879. He saw a broad white streak run- ning from Lake Pheonix in a N. N. E. direction, crossing the For- tuna and the doubled Nile, and seeming to join an extension of the polar snows. This white streak appeared to him to be ob- viously snow, (probably it was the track of a heavy snow-storm) and he examined carefully the place where it crossed the Nile, as he expected that if the latter were water, the snow would melt in it, and the streak be interrupted; while if the Nile were merely a marking on the land, the streak would of course pass across it without alteration. As a matter of fact, the Nile was not abso- lutely interrupted, but it was reduced to a mere thread running across the white streak. This observation seems to have been regarded as very puzzling. And yet, on the hypothesis that only the middle of the Nile is water, no other appearance could have been expected. This observation is, however, also explicable on the assumption that the Nile was frozen over, except in the mid-

dle; and the time of year, (shortly before the vernal equinox) is consistent with an assumption which involves extreme cold. The following, however, is not so easily explained.

These rivers, running down the middle of each ravine, might be expected to expand into lakes here and there, especially where two or more united. In some cases, these lakes would be less in diameter than the canal, and then, as seen from the Earth, the curious appearance would be presented of a small dark spot *in* the canal.

Such spots have been seen by Professor Pickering, and I cannot suggest any other explanation which is even reasonably probable.

It may be urged that if we can see the lakes, we ought also to be able to see the rivers themselves, as darker streaks down the middle of each canal. But it must be remembered that the lakes are probably in general, deeper than the rivers, and so look darker; (2), that the water of the rivers is more likely to be turbid, and so look lighter than that of the lakes; (3), that the river water, being in motion, may have its surface diversified with ripples, eddies, etc, which would make it look lighter from a distance. And, under very favorable circumstances, the middle of a canal *has* been seen darker than the edges.

As to the question whether the margins of a canal are clothed with vegetation, or merely have a darker color than the rest of the soil, either by being marshy, or any other reason, it is at present hardly safe to speculate; at the same time there is a good deal to be said for the hypothesis of vegetation. Firstly, if vegetation exists at all on the surface of Mars, along the banks of the rivers would seem the most likely place to look for it; secondly, the changes which have been observed in the appearances of the canals seem to point that way. As a single instance, take the Ambrosia; on the 26 September, 1877, one day before the summer solstice, this was seen to be broad and greyish; in November and December, 1879, it was seen as a fine, black line. This is exactly what we would expect if the fine, black line represents the *river* Ambrosia itself, and it is fringed by a broad belt of deciduous trees. At the time of the first observation, being the summer solstice, they were in full leaf; while at the second, which was shortly before the autumnal equinox, the leaves had changed color, or had fallen, so only the river itself was visible. This suggestion may seem to ascribe a degree of similarity to the Martian and Terrestrial vegetation which is *a priori* improbable; but having gone carefully through all the observations recorded

in "La Planète Mars," I have come to the conclusion that if vegetation exists at all on Mars, and the changes in appearance of the dark markings are due to this cause, it very closely resembles our terrestrial vegetation in the way it varies with the seasons.

The next matter to be considered is the duplication; this is generally regarded as optical, not in the sense of an illusion, but as being an unreal appearence, due to the meteorological conditions prevailing on the surface of the planet. Two explanations have been put forward; the first, due to Mr. Proctor, attributes the phenomenon to fogs resting on the rivers; the second, due to M. Meunier, ascribes it to a thin veil of cloud or fog between our eyes and the canals; he having found by experiment that a gauze screen held before lines, etc., duplicates them.

On independent grounds, I have seen reason to believe that there is a very thin cloud-screen at a height of between 10 and 20 miles from the surface, so that M. Meunier's may be the correct explanation. I doubt, however, if it is adequate to explain all cases; it seems to me, that there ought to be some reasonable proportion between the height of the screen and the apparent width of the duplication; and I do not exactly see how a cloud-screen, whose distance from the surface cannot much exceed 20 miles, can cause apparent duplications 200 or 300 miles wide. It seems to me more likely that only the narrower duplications are caused in this way, and that the wider are real.

A hypothesis to explain such cases, is, that of two canals, closely adjacent, and also parallel or nearly so, both draining the same area, one would come to be more used than the other; so in seasons of drought, the less used river would get choked up by stones, etc., falling into its bed, which the flow of water would not be sufficient to remove. So that in time there would only be one active channel. Then, in seasons of flood, the ordinary channel would not be sufficient to carry off all the water, consequently there would at first be flooding, evinced by a general haziness of aspect, until the water had succeeded in forcing its way down the second channel, and scouring it out, after which both canals would be visible.

This hypotheis is apparently inconsistent with the assumption that only the middle of the canals is water. Possibly they may be reconciled, or they may refer to different canals.

It will be seen that I regard the canals as the most ancient features of the surface of the planet. Not only that, but they are in a sense the key to all the others. They have determined the

distribution of sea and land, and fixed the course of the rivers from the first, so that only very insignificant variations have taken place, and traces of fluviatile action are confined to the canals. Moreover, by their acting as vents for the subterranean energy, traces of volcanic action are probably insignificant, and confined to the canals. Mountains are almost exclusively found along the margins of these latter, and, although some may be volcanic, it seems to me more likely that the majority simply result from the fragments of the crust being tilted up. In this case, their form would be totally unlike that of our terrestrial mountains; on one side there would be an abrupt fall, and on the other a gradual slope.

As there have been no upheavals, and no changes produced by fluviatile erosion, the only inportant changes which the surface of Mars has undergone in the course of ages have been those produced by the gradual recession of the sea. It seems not improbable that all the older continents are to a large extent desert, and that life, if it exists at all, is confined to the lands newly left bare by the sea, (as Hesperia, Atlantis, Libya, Thaumasia, etc., probably) and the canals. The fluviatile erosion in these latter must have been enormous, if the rivers have been running for millions of years in practically the same channels.

In the latter part of this, for brevity I have spoken dogmatically; it will of course be understood that these results are merely put forward as probable if the fundamental hypothesis (that the canals are cracks in the crusts of very ancient date) be granted.

DUBLIN, Ireland, 6 Harrington St.

THE POULKOWA DOUBLE STAR MEASURES.*

S. W. BURNHAM.

All who are interested in any way in the subject of double stars will welcome with more than ordinary interest the appearance of Vol. X of the Poulkowa observations. It has been in preparation for many years, and we have anxiously looked for its publication since the issue in 1878 of Vol. IX which contains the Poulkowa measures of the Σ and OΣ stars. These two volumes must be considered as among the most important contributions to double star literature since the publication of the original Σ

* Communicated by the author.

and OΣ catalogues. At no other Observatory in the world has this class of work been so systematically and continuously carried on. For three-quarters of a century, the Struves, father and son, at Dorpat and at Poulkowa, made this their life work. Fortunately they did not, as official heads of these great observatories, consider it an important or necessary part of their duties to direct the work. They went ahead for this long period of time and made the measures themselves; and the result is that in these splendid publications of the Struves, every observation from first to last is the personal work of one of these great observers. It is hardly necessary to say that this gives the measures a much higher scientific value than they would be entitled to if made in any other way.

The measures comprising the present volume are divided into four sections:

Section I embraces the observations of Herschel's Classes V and VI. These stars are taken from the Catalogues of Herschel, Struve and O. Struve. Some of these stars are more closely double, and measures of some of the closer pairs will be found in the other sections. Most of the distant components show very little change, although in some instances variation from proper motion is manifest. Many of the principal stars are prominent and well-known objects, such as α Cassiopeæ, λ Arietis, θ Aurigæ, ι Boötis, etc.

Section II deals with stars having large proper motions. Many of them are known as double stars by virtue of a near component traveling in space with the primary. The list includes most of the prominent stars known to be moving in this way, and as the measures connect with stars too remote for any probable connection with the primaries, the best possible data is furnished for a very exact correction of the movement as shown by the meridian observations. Nearly all the pominent stars having considerable proper motions are included in this section such as α Andromedæ, η Cassiopeiæ, 40 Eridani, α Tauri, Procyon, Castor, γ Leonis, 61 Cygni, etc.

Section III is devoted to stars of more modern discovery, and particularly to the β pairs. It includes also some of the discoveries of the Clarks, Dawes, Herschel and others. Most of the measures have been made during the the last twenty years. These pairs are nearly all of an interesting character.

Section IV embraces measures since 1875 of Σ and OΣ pairs. In many instances they are of special interest either from the rapid motion of the components, or because recent measures are

wanting. Among the binary systems we have 40 Eridani, 32 Orionis, ζ Cancri, ω Leonis, 42 Comæ, μ Herculis, φ Ursæ Majoris, OΣ 234, OΣ 536 and others of a like character. In this connection it is worthy of note that OΣ suspected in elongation of 72 Ophiuchi (OΣ 342) in 1877. This star has been examined many times in the last twenty years by various observers, including the writer, but without seeing any trace of the suspected companion. Evidently the star should be watched, since it is possible that it may be double, and in very rapid motion. At the end of the work we have an index to all the stars in volumes IX and X, arranged in order of R. A. To some extent this will do away with the practical inconvenience of referring to a work of this character where there is any classification of the double stars.

No one can more sincerely regret than the writer that this is probably the last extended contribution of the great Russian astronomer, so far as practical observations are concerned, to the literature of double stars. It would be difficult to overestimate the value of his services which cover a period of more than fifty years. His brilliant discoveries of remarkable binary systems, and his great skill as an observer will ensure to him a prominent and enduring place in all the astronomical records of the future.

RECENT OBSERVATIONS OF THE SATELLITES OF JUPITER.

EDWARD S. HOLDEN.

In ASTRONOMY AND ASTRO-PHYSICS for April, 1894 (page 272), and in the *Monthly Notices*, R. A. S., for January, 1894 (page 134), Dr. Barnard has two papers on his recent observations of the satellites of Jupiter. The following points are especially interesting in connection with previous observations on the same subject:

First.—The remarkable results announced in 1893 by Professor William Pickering with regard to the forms, rotations and behavior of these bodies are not confirmed by Dr. Barnard. In this respect his conclusions entirely agree with those of Professor Schaeberle, previously printed in the *Publications* A. S. P., Vol. V, page 182, dated September, 1893. In order that Professor Pickering's conclusions may be accepted, it will be necessary for him to verify the results he obtained in South America during 1892 by renewed observations, with a different object-glass, at his new station in Arizona, or elsewhere, and it seems to be re-

quired that some of the large instruments, now so plentiful, should confirm his conclusions.

Second.—In the *Publications* A. S. P., Vol. III, pages 355 and 359, Professors Schaeberle and Campbell described markings on Satellite III of Jupiter, and announced that Satellite I "is ellipsoidal, that its largest axis is directed towards the center of Jupiter, and that the other satellites appear to be spherical." Dr. Barnard's conclusions are that all the satellites, including I, are spherical. Professors Schaeberle and Campbell have continued their observations since 1891, and have so far seen no reason to change their conclusions above given, so far as I know. In the same paper they conclude that the periods of axial rotation and of revolution of satellite I are equal. Dr. Barnard says that his observations (as yet unpublished) lead to a different result.

Third.—The appearance of Satellite I in transit as a double body, as observed in 1890 by Professors Burnham and Barnard, is now explained by Dr. Barnard as due to simple contrasts between bright regions on the planet and two extensive dusky polar caps on the satellite (which are separated by a brighter belt).

The simple theory of contrast is probably fully adequate to explain all observed appearances of the phenomena of the transits of the satellites (dark transits, etc.), as has been pointed out in the *Publications* A. S. P., Vol. II (1890) page 296; Vol. III (1891), page 358, and in other places.

Fourth.—Dr. Barnard next considers the transparency of the limb of Jupiter with reference to the question whether the light of a satellite undergoing occultation can be seen through the planet's atmosphere, and says, "I think it is high time that astronomers reject the idea that the satellites of Jupiter can be seen through his limb" since under good conditions, with the 36-inch telescope, "the limb of Jupiter has appeared perfectly opaque, as in all my previous observations with smaller telescopes."

It is quite possible that the limb of Jupiter is really opaque to the light of satellites or stars, but it does not always appear to be so. Dr. Barnard has himself described the appearance of a star at occultation shining through the atmosphere of the planet (see *Astronomical Journal,*) Vol. VIII, page 64), though the observation had probably escaped his memory when he wrote the sentence just quoted. It is conceivable that the observations of satellites which he criticises were due to the same causes which affected his own observations of the star in question. At any rate his words "the star was last seen with three-quarters of its disc within the limb" of Jupiter, show that good observers have sometimes recorded appearances of the kind.

These few points from recent papers show very forcibly that everything is not yet settled with respect to Jupiter's satellite system, and may serve to direct the attention of the possessors of large telescopes to some of the problems involved.

Lick Observatory, April 10, 1894.

Astro-Physics.

ON THE SPECTRUM OF β LYRAE.*

H. C. VOGEL.

The variable star β Lyræ, remarkable through the peculiar form of its light curve, is to be counted among the most interesting spectroscopic objects in the northern heavens. The spectrum belongs to Class I, and extends far into the violet. In the visible portion it is crossed by individual bright lines, among which the hydrogen lines $H\gamma$ and $H\beta$ and the D_3 line are especially conspicuous.

The idea of connecting the bright lines in the spectrum of β Lyræ, which seemed to undergo changes of visibility, with the light-variation of the star, was one which naturally occurred to observers; nevertheless the various attempts which were made to discover such a relation led to no satisfactory and conclusive result, and a new interest in the star was first awakened by Pickering's communication (in A. N. 3051), announcing that photographs of the spectrum revealed the presence of closely adjacent bright and dark lines whose relative positions were subject to change.

Unfortunately, the prospect at the Potsdam Oservatory of being able to engage in work on this interesting object, was very slight, owing to the lack of an instrument which would give a spectrum of sufficient brightness. The star was too faint for the large spectrograph with which the motions of stars in the line of sight had been determined; and when instruments with less dispersion were connected with the 11-inch refractor, the effect of the visual correction of the telescope objective was at once apparent, for only a small part of the photographed spectrum appeared sharp, and the investigation must have been confined to essentially a single line. The success which had been obtained in an investigation of the spectrum of Nova Aurigæ, with a spectrograph of small dispersion used in connection with the 13-inch photographic refractor, justified the expectation that details could also be found in the spectrum of β Lyræ that would not be without importance in adding to our knowledge of the nature of this star, and I therefore took some pains to perfect the instrument which had served in the above-mentioned observations, and

* Translated from the *Sitzungsberichte der k. preuss. Akademie der Wissenschaften zu Berlin.* 1894. VI.

which had been constructed in only a provisional manner, and above all to increase its stability.

With the improved apparatus the spectra are of extraordinary sharpness. Owing to the fact that the collimator and camera objectives are achromatized for the same rays as the objective of the telescope, star spectra are obtained with this apparatus which are nearly equally sharp throughout the great extent of spectrum between λ 380 and λ 450. The dispersion is somewhat greater than with the earlier apparatus, in the ratio of about 5:4.

The material yielded by the observations up to the present time is, thanks to the zealous labors of Dr. Wilsing, quite considerable in amount. It is especially valuable for the reason that several photographs were taken on each observing night, with different exposures and breadth of spectrum, thus securing a result for the night which was as free as possible from accidental errors, and was in general dependent only upon the more or less favorable condition of the atmosphere. The times of exposure varied between 15 and 60 minutes; the breadth of the spectrum, produced by slightly altering the rate of the driving clock, varied between a mere line and a breadth of 0.4 mm. Photographs taken with a breadth of 0.2 mm, have proved to be the best, with respect to certainty of measurement and the recognition of fine details.

From March 25 to December 22, 1893, Dr. Wilsing made 144 photographs. Besides these, 7 plates were made on three evenings in November, 1892, by Dr. Wilsing, and 9, on nine evenings in April and May, 1892, by Professor Frost. Sixteen per cent of the total number of plates were unsuitable for a detailed investigation; among these are reckoned the plates with too short exposure, which are, however, instructive in a certain sense, inasmuch as they show the great importance of nearly correct exposures in the scientific application of photography, and how details, easily recognized on a sufficiently exposed plate, can be entirely lost on an under-exposed one taken on the same evening. The strong and disturbing influence of the state of the atmosphere, especially of a thin veil of haze, which in many other astronomical observations is so advantageous, was in general very noticeable in the photographs.

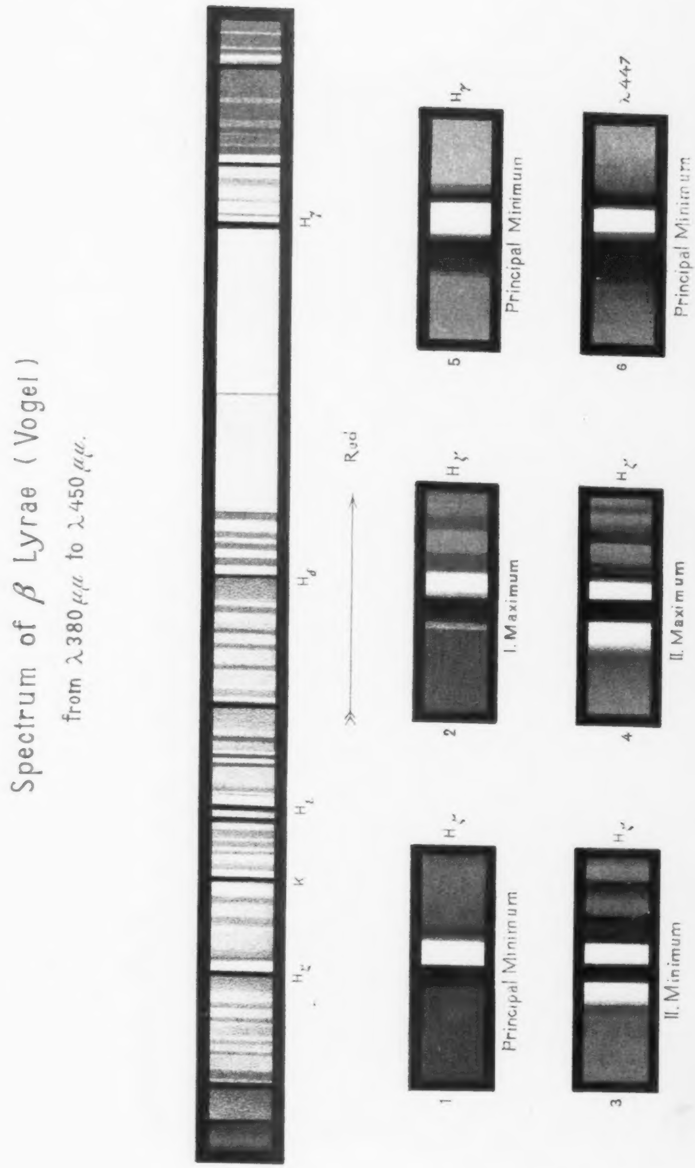
With regard to the spectrum of β Lyræ in general, measures of particularly suitable plates by Dr. Wilsing and myself show that the whole series of hydrogen lines from $H\gamma$ to $H\xi$ is present.*

* I here use for the first time the momenclature for the violet hydrogen lines which was proposed by me (A. N. 3198) and accepted by Huggins, instead of the earlier one by Huggins; I shall however, in the first examples, add in brackets the old symbol for the best-known lines.

The lines appear as broad, for the most part definitely bounded absorption bands. Besides the conspicuous line K, some other lines, resembling those of hydrogen, and several delicate lines, are visible in the spectrum. On the less refrangible side of nearly all the stronger absorption lines, bright lines sometimes appear, among which Hγ and Hζ (α) are especially noticeable.

The results of measures of plates taken on March 26, April 7 and 8, December 9 and 22, 1893, are collected in the following table. The wave-lengths have been determined with the aid of a table based on very numerous measures of solar spectra taken with the same apparatus. The uncertainty, between Hγ and Hη (β), is, liberally estimated, about, $\pm$ 0.15 $\mu\mu$. For lines near the limit of the photographed spectrum, which cannot be determined so accurately, the value given above is quite exact. It should not remain unmentioned that in the determination of wave-lengths the assumption has been made that the absorption line Hδ in the star spectrum coincides with this line in the spectrum of the artificial source, which has no motion relatively to the observer. It has not been possible hitherto to determine *absolute* wave-lengths in the star spectrum with greater accuracy then 0.15$\mu\mu$.

Micrometer. Reading.	Wave-length.	Description of Line.
r		
− 1.00	370.4$\mu\mu$	Weak absorption line at the limit of visibility. Hξ.
− 0.40	371.2	Weak absorption line. Hν.
+ 0.40	372.2	Weak absorption line. Hμ.
1.40	373.5	Weak absorption line. Hλ.
2.64	37.51	Absorption line. Hχ.
3.51	376.3	Delicate absorption line.
4.08	377.1	Broad absorption line. Hι.
{6.03	379.8	Absorption line. } Hθ.
{6.25	380.2	Bright line, very weak. }
{7.53	382.0	Absorption line. } Strong line in solar spectrum. L.
{7.75	382.3	Bright line. }
{8.62	383.6	Absorption line. } Hη.
{8.84	384.0	Bright line; very weak. }
10.23	386.0	Bright line.
{12.04	388.9	Absorption line; sharp. } Most conspicuous group of
{12.20	389.1	Bright line; very intense. } lines in the spectrum. Hζ (α).
{14.37	392.7	Absorption line; weak.
{14.59	393.1	Bright line; broad, diffuse.
{14.79	393.4	Strong absorption line. } K line of the solar spectrum
{14.98	393.8	Bright, conspicuous line. }
16.34	394.4	Bright line; very narrow, weak.
15.82	395.3	Bright line; broader than the above, weak, diffuse.
16.30	396.0	Bright line; broad, weak.
16.58	396.5	Strong, sharp absorption line.
{16.85	397.0	Broad absorption line; somewhat diffuse. } Solar H.
{17.09	397.5	Bright line; very broad, Hε. }
18.40	399.8	Bright line; narrow, weak.
18.58	400.1	Bright line; like the preceding.
{19.08	401.0	Delicate absorption line.
{19.26	401.4	Bright line; delicate, weak.
{19.44	401.7	Delicate absorption line.
{19.62	402.0	Bright line; delicate, weak.


Spectrum of β Lyrae (Vogel)
from λ380 μμ to λ450 μμ
Hγ
Hδ
Hε
K
Hζ
Red
1
Principal Minimum
2
I. Maximum
3
II. Minimum
4
II. Maximum
5
Principal Minimum
6
Principal Minimum
λ447

Micrometer Reading.	Wave-Lengths.	Description of Line.
r		
19.97	402.6	Absorption line; strong. } Conspicuous lines, resembling
20.17	403.0	Bright line; broad. } the hydrogen lines.
21.86	406.2	Absorption line. } Very faint.
21.96	406.4	Bright line.
22.15	406.8	Absorption line. } Very faint.
22.32	407.1	Bright line.
23.84	410.2	Absorption line; strong, broad. } Hδ.
24.03	410.6	Bright line; broad.
24.28	411.1	Absorption line. } Rather weak.
24.42	411.3	Bright line.
24.73	412.0	Absorption line. } Weak.
24.93	412.4	Bright line.
25.22:	413.0	Absorption line; narrow, weak.
25.40:	413.3	Bright line; weak, very wide.
25.83	414.3	Absorption line.
26.04	414.7	Bright line.
34.16	434.0	Broad absorption line. } Hγ
34 34	434.5	Broad bright line.
35.95	438.8	Absorption line; broad.
36.15	439.3	Bright line;broad, very weak.
38.93	447.0	Broad absorptionline. } Conspicuous lensis.
39.10	447.5	Bright line.
39.33	448.1	Absorption line; narrow, weak.
39.44	448.4	Bright line; very weak.

In the accompanying plate* I have also given a representation of the spectrum as it appears at the time of a principal minimum, as there has hitherto been published only a map of the visible part of the spectrum between D_3 and Hγ in the memoir on β Lyræ by Bélopolsky, to which these observations from an extension.

Changes in the spectrum, apparently having some relation to the period of the star, were immediately and easily recognized in the observations of March and April, 1893, which were made during an extraordinarily favorable period of weather. It first appeared, without any doubt whatever, that the intensity of the continuous spectrum varied with the light-phase, and that neither a variation of relative intensity in separate parts of the spectrum, nor a conspicuous change in the general nature of the spectrum, was immediately involved in the different light-phases of the star. It cannot be certainly determined whether the bright lines increase in brightness with the continuous spectrum or not; but in any case they do not merely make their appearance at the time when the star is brightest, as it has been asserted that they did. On the contrary, the photographs which have been taken here point rather to the opinion, that the bright lines do not take part in the periodic light-variation of the star, for they are most

* The plate is as close a reproduction as possible of Professor Vogel's plate in the *Sitzungsberichte.—Ed.*

conspicuous at the time of the principal minimum, very likely in consequence of the contrast with the weaker continuous background. The number of lines is also subject to no regular variation; it is constant for the principal lines, and the visibility of a considerable number of finer lines, bright as well as dark, which can be recognized at times, seems to depend only upon the particularly favorable atmospheric conditions under which they are obtained, or the excellence of the photograph. (Such especially good photographs lead to the supposition that with sufficient dispersion the spectrum would be found very rich in lines.)

Only small changes, undoubtedly standing in a certain relation to the periodic variations of the star, can be observed in the relative positions of the bright and dark lines, which for the most part are arranged in pairs throughout the spectrum. It appears that at the time of a principal minimum the absorption lines come out very distinctly, and the bright lines lie close to their less refrangible sides. At the time of a maximum the absorption lines are less distinct; at the time of the first maximum the bright lines are still on the side toward the red; at the time of the second maximum they fall on the absorption lines, (which in consequence appear very narrow), and assume the aspect of bright lines situated one on each side of the dark line, although on account of the greater brightness of the continuous spectrum they are often hard to see. The aspect of the lines in the intermediate minimum is nearly the same as at the time of the second maximum, but the absorption lines are broader and more conspicuous.

The above observations apply to most of the stronger lines between λ 385$\mu\mu$ and λ 450$\mu\mu$, and not to the whole spectrum shown on the plate; for the intensity falls off very noticeably at λ 385 if the correct exposure has been given to the other parts; and no reliable statements, based on the plates which are at present available, can be made as to the behavior of the more refrangible hydrogen lines. Some of the plates show that in this part of the spectrum also, bright lines are at times situated close to the absorption lines, while at other times the bright lines fall on the absorption lines and cause them to appear very faint.

The phenomena described above appear in a much more pregnant manner in the case of the Hζ line. This is by far the most striking line in the photographic spectrum. While some of the bright lines are at times scarcely percepitble, the bright line Hζ is always plainly evident; in poor photographs it is often the only feature in the spectrum worthy of remark. The bright line is narrower than the other lines of hydrogen; the absorption line is al-

ways sharply bounded, and it is outlined with quite especial sharpness when apparently enclosed by the bright line. It is remarkable that the absorption line is absolutely dark; in the negative, forming a spot free from the slightest deposit of silver, it is often brightly transparent. This appearance may in part be due to the peculiarity of photographic processes, that contrasts brought out by powerful development are stronger than they are in reality; but the behavior of this line as compared with that of the other lines in the spectrum remains a remarkable fact, well worthy of special consideration.

Although I do not regard our observations of β Lyræ as completed, I have sought to collect all the hitherto observed phenomena relating to the $H\zeta$ line in a preliminary manner, in order to secure a basis for further investigations, and give them here in some detail, as they may also be of some importance to other observers. The observed changes are noted with reference to the phase of the star's period, since as I have said, the first observations revealed a certain connection between the two phenomena.

A. 1. At the time of the principal minimum and the maximum following it, the bright line lies close to the dark one, displaced in the direction of greater wave-length. Occasionally,—particularly at the time of the first maximum,—the dark line is bounded on the upper side also by a bright but quite narrow line. (Fig 1 and 2, 1893. April 7 and April 10).

2. At the second minimum the absorption line lies in the middle of the bright line, in such a manner, however, that the red component of the bright line, which then appears as a wide double, exceeds the other in breadth. On the less refrangible side of the bright line appears a weak absorption line.

3. At the second maximum the absorption line lies either exactly in the middle of the bright line, or toward one side, so that the more refrangible component is somewhat the broader. The above-mentioned absorption line on the less refrangible side of the bright line, is very distinct, and quite characteristic of the appearance at the time of the second maximum. Occasionally two or three more lines can be recognized, at a short distance toward the red. (Fig. 4, 1893, April 4).

4. While the passage from the first maximum to the second minimum is a quite gradual one, the change in the aspect of the double $H\zeta$ line between the second maximum and the principal minimum is very sudden,—indeed, it occurs shortly before the advent of the principal minimum.

B. The aspect of the lines described above, which is that

shown on most of the photographs, is sometimes very perceptibly different. At the principal minimum and first maximum the bright line does not lie entirely on one side of the absorption line, but encloses it, as in the case of the second minimum (A, 2) although the red component of the bright line is considerably broader. At the time of the second minimum and the following maximum, the more refrangible component of the bright line is considerably broader than the other component, and on the photographs of Oct. 13, 1893, (between the second minimum and the second maximum) the bright line can be seen on only one side of the absorption line, displaced in the direction of smaller wavelengths, and hence the arrangement is just the reverse of what it is (A, 1) at the time of the principal minimum.

The differences mentioned above are not, however, peculiar to the $H\zeta$ line, but are found in the other lines also. Thus on the plate of Oct. 13 several of the absorption lines have bright lines in their more refrangible sides. The impression produced is that the bright lines are displaced with reference to the dark ones throughout the whole spectrum.

The series of observations at the times of these conditions of the double lines in the spectrum of β Lyræ is still defective, but there is little doubt that further observations will enable us to succeed in discovering the cause of these hitherto unknown or unnoticed changes which may, be supposed to require a longer period, perhaps one of several months.

C. Not only the bright line $H\zeta$, but the other strong bright lines, undergo a change in breadth and intensity which must probably be regarded as real, since it can hardly be ascribed to atmospheric conditions or length of exposure. Whether the change is periodic or entirely irregular we have not yet been able to determine; but at any rate, as mentioned above, it stands in no immediate relation to the light-phase. That the dark lines also show large variation in breadth and intensity has already been mentioned; the breadth depends greatly upon the situation of the bright lines —whether they appear on both sides of the absorption lines or on one side only; but the length of exposure has undoubtedly in this case a great influence, which it is difficult to determine.

I give here a few measurements of the breadth of the $H\zeta$ line, in order to show the limits within which the change of breadth takes place.

1893.	Breadth of absorp. line.	Breadth of bright line.	Remarks.
	r	r	
April 8	0.13	0.24	The bright line lies on the red side of the dark one. On the other side of the absorption line is visible only a quite fine bright line there 0.03r ro 0.04r in breadth.
April 17	0.15	0.45 {0.15 / 0.15	The absorption line lies in the middle.
April 23	0.11	0.20	The bright line lies on the red side.
Oct. 13	0.10	0.15	The bright line lies on the violet side.
Nov. 10	0.13	0.51 {0.19 / 0.19	The absorption line lies in the middle of the bright one.
Nov. 21	0.12	0.33 {0.15 / 0.06	The absorption line within the bright one, but displaced from the middle toward the red.
Dec. 23	0.11	0.20	The bright line lies on the red side.

The measures are given in micrometer revolutions; $0.1r$ corresponds to a change of wave-length of $0.16\mu\mu$.

It is a matter of special interest to determine the amount of the relative displacement of the lines, since this displacement can hardly be otherwise regarded than as resulting from the motion of different bodies, having dissimilar spectra which are superposed on account of the small distance between the bodies. Unfortunately it has not yet been possible to make absolute determinations of the positions of the lines with sufficient accuracy; but I hope that further observations can be made complete in this direction also.

The table on page 366 shows that at the time of greatest displacement the distance between the centers of the bright and dark lines of a pair is in the average 0.2 rev.; this would correspond to the very considerable motion of about 300 kilometers per second; but a more critical consideration of the phenomena and comparisons of the different lines with one another, show that the determination of a velocity by simple measurement of the distance between the bright and dark lines cannot be regarded as reliable since the bright lines undoubtedly partially overlap the dark ones, even at the time of their greatest separation. The extent to which this partial overlapping influences the two lines depends upon the breadth and intensity of the bright line, the form of its intensity-curve, and also upon the intensity-curve of the dark line. The intensity of the continuous spectrum, which varies with the light phase of the star, also plays a considerable part in this connection, and explains some minor differences which are found in the different lines of one and the same spectrum.

It has sometimes been obssrved that the broad absorption line $H\gamma$ completely encloses the bright line, so that the absorption line appears on each side of bright one, with the two parts of

greatly unequal width, while the bright Hζ line lies entirely on one side of its dark companion. At the times of the second minimum or second maximum it happens that the absorption line Hγ is so completely covered by the bright line that it is hardly distinguishable from the continuous spectrum. If, however, the bright line is at this time broader than the absorption line, narrow bright lines are produced at the borders of the hidden absorbtion lines as in the case of Hζ. It will be seen from this that the spectrograms require a very close and critical study if erroneous conclusions are to be avoided, which may easily be drawn from appearances that seem to differ for different pairs of lines, but are really explicable on simple principles, and referable to the same special case.

The case first mentioned, where at the time of greatest displacement the bright line lies within the absorption line, and which was several times observed in Hγ (Fig. 5) and in the line λ 477$\mu\mu$ (Fig. 6)*, affords the possibility of obtaining a fairly close approximation to the magnitude of the displacement of the lines and consequently to the amount of motion. I give below a number of measurements which I have made on two plates taken on December 22, 1893. The readings given are means of settings on the edges of the lines or on their centers.

Line Hγ.

0.1 rev. corresponds to a difference in wave-length of 0.26$\mu\mu$.

0.1$\mu\mu$ corresponds to a velocity of 69 kilometres.

Plate I.		
Absorption line	0.747r, 1.227	Distance = 0.077r.
Bright line, middle.	1.064	

Plate I.		
Absorption line	0.750r, 1.217	Distance = 0.075r.
Bright line	0.948, 1.169	

Plate II.		
Absorption line	0.471r, 0.976	Distance = 0.097r.
Bright line, middle	0.821	

Line λ 447.

0.1 rev. corresponds to a difference in wave-length of 0.28$\mu\mu$.

0.1$\mu\mu$ corresponds to a velocity of 67 kilometres.

Plate I.		
Absorp. line	0.179r, 0.749	Distance = 0.070r.
Bright line, middle.	0.534	

Plate I.		
Absorp. line	0.177r, 0.771	Distance = 0.067r
Bright line	0.451, 0.631	

Plate II.		
Absorp. line	0.214r, 0.769	Distance = 0.070r
Bright line, middle	0.562	

* In the figure representing the line λ 447, the closely neighboring pair of dark and bright lines at λ 448, which is not easily recognized on all the plates, has been omitted, in order that the appearance of the line λ 477 may be more closely shown.

Plate II.

$$\text{Absorp. line}\begin{cases}0.198r\\0.765\end{cases}\quad\text{Bright line}\begin{cases}0.411\\0.739\end{cases}\quad \text{Distance} = 0.093r$$

The mean gives for $H\gamma$ a displacement $= 0.083r$, corresponding to $0.216\mu\mu$ or a velocity of 149 kilometres; for the line λ 447 the displacement is $0.075r$, corresponding to $0.210\mu\mu$, or a velocity of 140 kilometres.

(TO BE CONTINUED).

ON THE INFLUENCE OF THE SLIT-WIDTH ON THE APPEARANCE OF COMET SPECTRA.*

H. KAYSER.

It was quite early assumed that the spectra of comets consist essentially of bands which are seen when compounds of carbon are burned or rendered incandescent by electric discharges, notwithstanding the fact that considerable differences were indicated by the observations. The most important of these differences are the following:

(1). Each of the groups of bands in the carbon spectrum (for it is now quite certain that the spectrum of carbon itself is concerned, and not that of a hydrocarbon), begins with a maximum of brightness on the red side; in cometary spectra, on the other hand, the maximum is within the band, and therefore displaced toward the violet.

(2). The wave-lengths of the maxima in cometary spectra are always found to be smaller than the wave-lengths of the heads of the carbon bands; the wave-lengths of the edges of the cometary bands are likewise smaller than the edges of the carbon bands; only in the case of the brighter cometary band at λ 516 is the wave-length of the beginning frequently found to be greater.

(3). Whenever a dispersion could be employed sufficiently great to show the second or third maximum of the bands in the comet spectrum, as well as the first, these maxima were also displaced toward the violet.

(4). While in each carbon band the first maximum is the brightest, and the following maxima become gradually fainter, in comets it has frequently been observed that the second maximum is brighter than the first.

* Translated from A. N. 3217.

(5). A series of observations was made by Harkness on Comet Encke 1871, V, according to which the wave-lengths of the maxima were greater, the greater the brightness of the comet.

(6). Measures by different observers on the same comet exhibit extraordinary differences, which often far exceed the possible limits of error.

All these anomalies in cometary spectra have remained unexplained up to the present time, although most of them have frequently been pointed out, and (as for example in the excellent collection of measurements by Hasselberg*) placed clearly before the eyes of observers. Only two attempts have been made to explain at least some of these anomalies. H. C. Vogel observed the mixed spectra of carbon and carbon monoxide. The CO band at λ 5609 then fell within the beginning of the C band at λ 5634, apparently producing a maximum of the C band at the former place. Further, the CO band at λ 5198 fell before the third C band at λ 5164, and thereby produced an apparent displacement of the beginning of the C band toward the red. Finally, a weak CO band at λ 4698 fell within the fourth C band at λ 4737, causing an apparent displacement of the maximum of the C band toward the violet. Vogel therefore makes the assumption that the spectrum of a comet often consists of the superposed spectra of C and CO, and in this manner seeks to explain the displacement of the maxima.

This hypothesis appears at first sight to accord quite well with the facts of observation; nevertheless it seems to me inadmisible for two reasons:

(1). If some of the CO bands appear in comet-spectra, then since one of the weaker bands is supposed to be strong enough to answer the requirements of the explanation, the other bands, at least the strongest, as for example those at λ 6079, 4834 and 4510 should be seen. This is however not the case.

(2). One would at least expect that the different measures would show some correspondence with the appearance of the bands in the mixed spectrum; for instance, that the apparent maximum of the third C band would always be found near λ 5609. But there is absolutely no hint of this in the observation; thus three measures by Vogel himself give for comet Encke 1871 V ; λ 5552, for 1871 IV; λ 5571, for 1874 III: λ 5538.

The second attempt to furnish an explanation is due to Hasselberg. He has found that when very weak electrical discharges

* Mém. de l' Acad. de St. Petersb. Ser. VII, Bd. 28, No. 2.

are sent through carburetted hydrogen in Geissler tubes, the second maximum of the fourth carbon band (and perhaps also that of the second) can under certain circumstances become brighter than the first maximum. But even when it is assumed that this experiment is quite free from objection, and that the appearance is perhaps not due to CO, as in the case of Vogel's experiments, (and Hasselberg says himself that the CO spectrum was present at the same time), it explains only one of the anomalies which have been observed in the spectra of comets.

It must be said, therefore, that the measures of the spectra of comets have as yet found no explanation. Hasselberg constructs a "type of cometary spectra" which differs from the carbon spectrum, and Scheiner even says (*Spectralanalyse der Gestirne*, p. 227,) that the distance of the maximum from the edge of the band is perhaps really variable in cometary spectra, and for this reason recommends that the edge of the band and not the maximum, should be measured.

I believe, however, that such an assumption, which is in contradiction to all the fundamental principles of spectrum analysis, is unnecessary, and that all these anomalies admit an extremely simple explanation. It is, very probably, exactly this great simplicity that has caused this explanation to be so long overlooked. All the observed phenomena follow from this circumstance; that the comet spectrum is extremely faint, and hence the observer is compelled to use a low dispersion and a wide slit. He will always make the slit as narrow as possible, and will therefore choose different slit-widths, according to the birghtness of the comet and the character of his instrument. *This varying slit-width is the cause of all the observed anomalies*, as I shall proceed to show.*

In every spectrum a spectral line is an image of the slit. If, as is customary in spectrometers, the lenses of the collimator and the observing telescope have the same focal length the breadth

* In the work by Scheiner, mentioned above, are a number of remarks on the influence of the slit-width which are not clear to me. Schiener says (page 226), "With a widely opened slit the sharp edge of such a band is of uniform intensity within the breadth of the image of the slit, and the observer, in setting on the edge of the band, has therefore a tendency to select, not the edge itself, but the middle of the uniform brightness, by which an error in setting of half the slit-width toward the violet is caused." As I will show, the breadth of the image of the slit is not of uniform brightness and all that follows rests exactly on this point. The second half of the sentence I do not understand. If the image of the edge were actually of "uniform brightness," the setting on its center would give a measurement entirely correct; provided only that the standard lines, for determining the wave-lengths from the measures, were measured with the same slit-width, and that the middle of the image was also taken in their case. But this requirement is so obvious that Scheiner could not assume that it was neglected by the observer.

of the line is the same as that of the slit, (widened lines being of course left out of consideration). In what follows it will be assumed for the sake of simplicity that this is the case. * The breadth of the line therefore increases with the width of the slit. This is no detriment to measurement in a line-spectrum, if only the settings are made on the same parts of all the broad images—center, edge, or any other part the observer chooses.

But the case is quite different when many lines lie close together, as in bands or flutings; by the superposition of the broad images the image of a single line disappears; we obtain in the spectrum only an increasing and diminishing intensity, which at any place represents the sum of the lines that fall there. In order to obtain conveniently a clear view of the effect produced, let us imagine the whole spectrum, as projected with a narrow slit, (and for the sake of brevity I will call this the true spectrum, that given by a wide slit the apparent spectrum), divided into narrow strips, each of which we may regard as having a constant mean intensity. The breadth of each strip we may take to be, say, 2 Angstrom units. Now let the slit-width be equal to three such strips; then on every strip of the apparent spectrum fall the images of three strips of the true spectrum, and we obtain the intensity of a strip of the apparent spectrum by taking in the true one the sum of its intensity and that of the two neighboring strips.

Similiar considerations apply to every slit-width, for which the apparent spectrum may be found thus: consider a slit of the chosen width to be pushed over the true spectrum successively from strip to strip; for each position take the sum of the intensities of all the strips covered by the slit, and the result is the intensity in the apparent spectrum of the strip which occupies the middle of the slit.

It is now very easy to understand the conditions if we assume that we are dealing with a band, which has only one sharp edge, on (say) the red side, and which falls off gradually toward the violet. We allow the slit S, whose breadth is equal to n strips, to pass over the true spectrum, and draw the apparent spectrum in accordance with the foregoing principles. We obtain an intensity for the first time when the advancing edge of the slit has passed over the first stip of the true spectrum, and the intensity

* In reality the observing telescope naturally does not come into consideration, but only the angle subtended by the edges of the slit as seen from the center of the collimator objective. The focal length of the telescope is a matter of indifference, since the dispersion and the breadth of the slit-image vary together; thus with any focal length the same number of Angstrom's units are included in the image. It is the number alone which is concerned.

is that of this strip. Thus the beginning of the apparent band is displaced toward the red by ½ *S*. As the slit passes on, two, three, etc., strips of the true spectrum gradually fall within its limits, and the intensity of the apparent band thus gradually increases; but continually at a slower rate, since the strips which are added are always of diminishing intensity. As soon as the slit lies for the first time in the true band, the highest possible maximum of intensity is reached, for any further motion brings in strips on the violet side which are weaker than those which pass out on the red.

Thus we find that our apparent band begins ½*S* further toward the red, gradually increases, reaches a maximum which is displaced ½*S* toward the violet, as compared with the edge (and maximum) of the real band, then gradually falls off.

Very much more complicated are the relations when, as in the case of the carbon bands, we have to deal not with a single band but with a group of bands, having several edges, the intervals between which diminish in passing from red toward the violet and are filled with light from the gradually weakening bands. As long as the slit is still narrower than the average distance between the edges, the foregoing considerations hold; the beginning of the whole group of bands is displaced ½*S* toward the red, and every maximum ½*S* toward the violet. But if we take the slit wider—as wide say, as the distance between the first two edges—and again imagine *S* to be entering the true band, then the intensity of the apparent band will begin to increase from zero, and will reach its first maximum when the advancing edge of *S* exactly touches the second edge. If *S* moves further toward the violet, the first, second, . . . strips of the first partial band will drop out, but the first, second, . . . strips of the second partial band will come in. If now, as actually is the case in the carbon bands, the second, and likewise the third partial band are only slightly weaker than the first, the motion of the slit will bring in strips on the violet side of nearly the same intensity as those which leave it on the read; *i. e.*, the intensity of the apparent spectrum over a certain distance will diminish very gradually. Since the distance of the third edge from the second is less than that of the second from the first, the intensity will increase rapidly, on continuing the motion of the slit, as soon as the advancing side reaches the third edge of the band; for the strips which leave the slit on the red side are last and faintest of the first partial band, while those which enter on the violet side are the first and strongest of the third partial band. The second

maximum of the apparent band is reached when the following side of the slit reaches the second edge, and this maximum is greater than the first since two maxima of the true band contribute to its intensity. In like manner further maxima are obtained whenever the following side of the slit reaches an edge of the true band.

The result is then this: that we see a group of bands with several edges, but which has the following peculiarities:

(1). The band is also diffuse on the side toward the red;

(2). The edges are displaced ½*S* toward the violet;

(3). The first maximum is less insense than the maxima which follow it.

If the slit is allowed to become still wider, the following conditions still obtain:

(1). The beginning of the entire band is always displaced ½*S* toward the red;

(2). The maxima are always displaced ½*S* toward the violet.

But these maxima gradually become less pronounced as the slit widens, since in the great number of strips which are to be summed up, the effect of any single strip is less. No general statements can be made with regard to the relative intensities of the different maxima. The anomaly of the apparent spectrum, that the following maxima are greater than the first may exist, or it may be caused to vanish, according to the assumptions which are made as to the rate of diminution of intensity in the edges of the bands in the true spectrum. In any case it is less pronounced.

Since now the distances between the edges of the partial bands are different in the different carbon groups of the cometary spectrum, and since above all the dispersion in the prismatic spectrum is different for each group, it follows that these anomalies may or may not exist for the different groups according to the width of the slit. Since with a wide slit the maxima are but little stronger than their surroundings, they may easily escape detection, and the middle of the nearly uniformly bright central portion may be taken for the maximum. The displacement measured in this way from the first edge naturally does not then represent the half slit-width, but is much greater.

If, finally, the slit is so wide that it embraces all the edges of the band in the true spectrum, the apparent spectrum will have only one maxmum, displaced by ½*S* from the true beginning of the group toward the violet.

The above considerations will be made clearer if I apply them numerically to a definite carbon band. For this purpose I shall

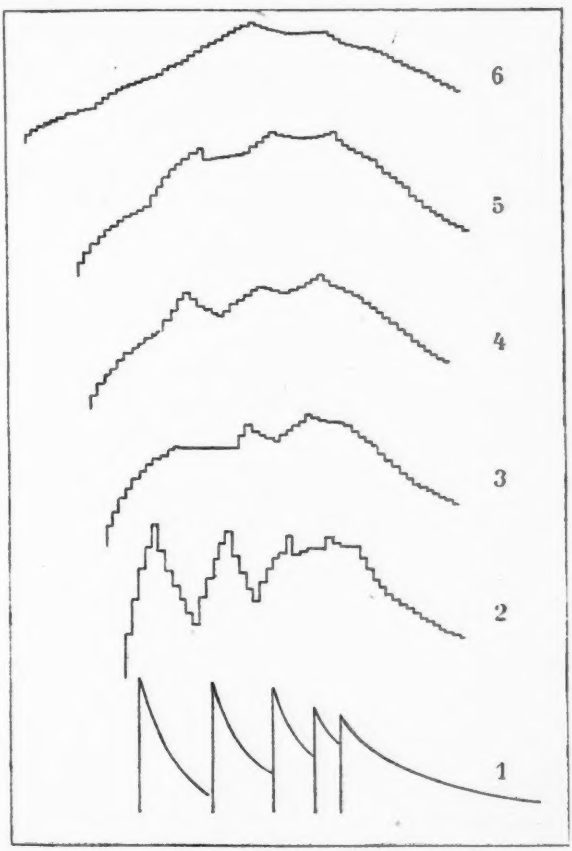
choose the fourth group of bands, whose edges are, in round numbers, at λ 4737, 4715, 4697, 4685, and 4677. I shall consider the spectrum to be drawn on the normal scale; the ordinates represent intensities, and the spectrum is divided into strips each two Angstrom's units wide. The mean intensity of each strip must now be estimated, a task which it is difficult to perform with accuracy, but which I have undertaken to perform as well as possible, with the aid of photographs, many of which were taken by myself and some by others. I shall represent the intensity of the first edge by 20, and, on this scale, estimate the intensities of the following edges to be 19, 18, 16, and 14. In the following table is given the intensity of each strip into which the spectrum is divided. The strips are numbered by calling the strip corresponding to the first edge 0, those following in the direction of the violet 1, 2, ...; and those toward the red $-1, -2$, ... The table shows then the distribution of intensity, (1) with a narrow slit; (2) with a slit whose width is five strips = 10 Angstrom's units; (3) eleven strips = 22 A. U.; (4) fifteen strips = 30 A, U.; (5) nineteen strips = 38 A. U.; (6) 35 strips = 70 A. U. In each case the maxima are made conspicuous by heavy-faced type.

In the accompanying figure these intensities are shown for the six different cases; but the ordinates of 2 are reduced to $\frac{1}{3}$, those of 3 to $\frac{1}{7}$, those of 4 to $\frac{1}{9}$, those of 5 to $\frac{1}{10}$, and those of 6 to $\frac{1}{20}$, in order to make the figure sufficiently small. Thus the curves give the aspect of the group of bands as seen with their corresponding slit-widths.

The table and the drawing show with a slit-width of 10 Angström's units all five maxima, but each is displaced 5 A. U. towards theviolet. The slit-width of 22 A. U. shows only the first three maxima each displaced 11 A. U. in the same direction; but the second maximum is 21 per cent brighter than the first, and the third 34 per cent brighter. The slit-width of 30 A. U. also gives three maxima each displaced 15 A. U., but the second is only 4 per cent brighter than the first and the third 14 per cent brighter. With a slit-width of 38 A. U. the second maximum is the highest of the three which are present, but it is only a little higher than the first and third. These last however are so little elevated above their surroundings that they would hardly be visible, and it is probable that only the second maximum would be measured, thus showing a displacement of 40 A. U. with a slit only 38 A. U. wide. The last spectrum, finally, shows only one maximum, displaced by half the slit width; of the second there is present only an indication, which would be practically invisible.

Number of the Strip.	[1] Narrow slit.	[2] Slit=10 A.U.	[3] Slit = 22 A.U.	[4] Slit = 30 A.U.	[5] Slit = 38 A.U.	[6] Slit=70 A.U.
− 10	0	0	0	0	0	91
− 9	0	0	0	0	20	96
− 8	0	0	0	0	36	100
− 7	0	0	0	20	49	103
− 6	0	0	0	36	60	122
− 5	0	0	20	49	70	137
− 4	0	0	36	60	78	150
− 3	0	0	49	70	85	161
− 2	0	20	60	78	91	170
− 1	0	36	70	85	96	178
0	**20**	49	78	91	100	185
1	16	60	85	96	103	191
2	13	**70**	91	100	122	196
3	11	58	96	103	137	204
4	10	49	100	122	150	218
5	8	42	**103**	137	161	230
6	7	36	102	150	170	241
7	6	30	101	**161**	178	251
8	5	35	101	150	185	260
9	4	37	101	142	**191**	276
10	3	46	100	136	176	289
11	**19**	54	100	131	178	300
12	15	61	100	126	179	310
13	13	**67**	100	136	180	324
14	11	56	100	143	181	337
15	9	48	114	149	183	345
16	8	41	**125**	155	185	356
17	7	35	118	161	195	**366**
18	6	44	114	**167**	203	355
19	5	50	111	164	210	347
20	**18**	55	109	162	**217**	342
21	14	60	116	160	212	338
22	12	**65**	121	159	210	335
23	11	56	125	164	208	333
24	10	58	129	169	208	332
25	9	59	**138**	174	209	332
26	**16**	59	133	179	210	333
27	13	59	131	**184**	211	335
28	11	**64**	130	175	213	320
29	10	61	129	169	**215**	309
30	**14**	60	128	165	204	300
31	13	60	127	161	196	293
32	12	60	119	158	190	287
33	11	55	113	155	185	282
34	10	50	109	145	180	278
35	9	46	105	138	176	275
36	8	42	97	132	165	272
37	8	39	90	127	156	256
38	7	36	83	118	149	244
39	7	34	77	109	143	234
40	7	32	72	101	133	225
41	6	30	67	94	123	217
42	6	28	63	88	114	210
43	6	27	59	82	106	198
44	5	25	56	77	99	187
45	5	23	52	72	92	177

The spectrum of a comet will differ from the appearances described above, in that the beginning of the band will not seem to be displaced ½ S towards the red. As we have seen, the beginning of the apparent band has the same brightness as the true edge of the band with a narrow slit. The slit is however made wide, merely because this brightness is not sufficient; it follows therefore that the beginning of the apparent band is also invisible,



and with the same slit-width, the place at which the band seems to begin will depend only upon the brightness of the comet, the character of the instrument, and the weather. There is no object, therefore, in measuring this place of beginning unless it be for the

purpose of estimating intensities. On the other hand, the position of the apparent maximum depends only upon the apparatus, and from it the wave-lengths of the true edges can be computed when the dispersion and slit-width are known.

It seems to be hardly necessary that I should make a detailed application of the above discussion to the case of cometary spectra. According to the light-gathering power of the telescope and the dispersion used, observers have chosen the width of the slit, and have thus measured quite differently the position of the maxima; it would seem that they have usually been obliged to employ a slit-width corresponding to the fifth or sixth example above, in which each group of bands shows only one maximum. If the comet became brighter, the slit was gradually made narrower, causing a progression of all the maxima toward the red—the observation of Harkness. If the slit could be made so narrow that several maxima became visible, that width might accidentally be hit upon which would make the following maxima weaker than the first. It will be seen that all the observations are explained naturally; and thus a hitherto somewhat unpleasant chapter in the subject of cometary spectra is elucidated in the most beautiful manner.

It only remains that I should make a numerical comparison of my explanation with the results of observation. This is only in a very limited degree possible, since I have found no data regarding the slit-width and dispersion of the instruments which have been employed by observers. Several points may, however, be brought out.

With a strongly dispersive flint glass prism of 60°, having a collimator and a telescope of 25 centimetres focus, I have measured the dispersion at the places of the three principal carbon bands. I found that at λ 563 in the spectrum 230 A. U. fall on one millimetre, at λ 516, 160 A. U., and at λ 437, 110 A. U. Although different prisms, and particularly those with direct vision, may have sensibly different relations in this respect, the numbers given above nevertheless supply us with a certain foothold, and we may expect that the observed displacements of the maxima in the three bands will bear the relation to each other of about 230: 160: 110, or in round numbers 4: 3: 2. Hasselberg has computed the mean values from all observations up to 1880, and finds for the maxima: 5564, 5127, 4705, and these compared with the true values 5634, 5164, 4737 give the differences 70, 37, 32. It seems to me that when the uncertainties of measurement are considered, this is not a bad result. The middle band only

gives a displacement which is too small, as it should be about 50. The result that the middle band is often less incorrectly measured than the others, is even more evident when the separate measurements are regarded; those collected by Hasselberg as well as those which have been made later. From them it would almost appear as if some of the observers had measured the middle band with a narrower slit; this would be explained by the greater brightness of the middle band, allowing the use of a narrower slit. According to numerous observations by Vogel, Lindsay, Tacchini, v. Konkoly, v. Gothard, the relative intensities of the three bands (somewhat different in different comets) are about as 4: 10: 3. When, for example, in the comet 1881 III, Harkness finds the displacements 141, 40, 65, Vogel, on the other hand, 94, 74, 39, this seems to me to indicate that Harkness measured the middle band with a narrower slit, and Vogel, more correctly, with a slit of the same width as that employed for the other two bands.

The measurements of the visible beginnings of the bands also agree quite well. The beginning of the middle and brightest band is often seen to have a greater wave-length than that of the true beginning. Hasselberg's mean value makes the beginning of this band 4 A. U. too great, that of the two other and much fainter bands 10 and 8 A. U. too small.

The amount of the observed displacement leads also to not improbable slit widths. The displacement of the first band is seldom as much as 100 A. N. With my apparatus this would correspond to a slit-width of 0.8 mm., but with a smaller dispersion, such as no doubt has generally been employed in observations of comets, to a slit much narrower than this. The displacement is in general, however, materially smaller, so that it would correspond to a slit width of 0.1 to 0.3 mm. In those cases where several maxima were seen, the displacement is small since a moderately narrow slit is essential in such an observation.

A more definite proof of my explanation would be possible if observers of comet spectra would publish the data regarding the dispersion of their instruments and the slit-width employed by them, as well as any changes which were perhaps made. But so far as one can judge without this information, I find no contradiction between my conclusions and experience. In the mean time the best confirmation seems to me to lie in this,—that in recent measurements, whose heightened accuracy we owe in part to larger telescopes, but particularly to photography, all these anomalies disappear, and the cometary spectrum appears as a true carbon

spectrum, with bands of exactly the same position and structure as those which we observe in our laboratories.* Without the explanation of the origin of these anomalies, no proof would however yet have been brought forward that the earlier comets also gave the ordinary carbon spectrum.

In closing I wish to mention that the appearences which I have described can be observed without difficulty in the laboratory. The experiment is particularly beautiful when a spectrometer is used with a Kruss double slit, the jaws of which widen symmetrically. If the carbon arc is projected on the double slit, and one of the slits is opened, it will be seen how isolated lines, for instance those of calcium and iron, which are always visible in the carbon arc, gradually widen, but have their centres always in the same position with reference to the narrow lines generated by the second narrow slit. The edges of the carbon bands, on the contrary, not only widen, but the maxima travel in the direction of the shorter wave-lengths. It is also easy to choose the breadth so that the first maximum is weaker than the second.

THE THERMAL RADIATION FROM SUNSPOTS.†

W. E. WILSON.

These observations were made by means of a large heliostat, lent by the Royal Society, and a Boys's radio-micrometer. The heliostat consists of a plane silver-on-glass mirror of 15 in. aperture. It is mounted equatorially, and driven by a clock. When in use, it is adjusted to reflect the sunlight to the north pole, and, as long as the driving clock is kept in motion, the beam of light remains fixed in that position. In the track of this beam, and about 12 ft. from the plane mirror, is mounted a concave silver-on-glass mirror of 9 in. aperture, and about 13 ft. focus. Its axis points to the south pole, so that the cone of rays formed by it strikes the centre of the plane mirror, and a short distance inside the focus. A small plane mirror mounted on the end of an arm is then so placed as to intercept the cone of rays, and reflect it hori-

* The excellent measures of Campbell which I recently noticed (A. N. 3214, ASTRONOMY AND ASTRO-PHYSICS, March, 1894) also show that all the edges of the bands have a wave-lengtn 1 or 2 A. N. too small, Campbell himself directs attention to this, but he does not know the reason for it. I conjecture that his slit width must have corresponded to about 3 Angstrom's units.

† Preliminary Notes of Observations made at Daramona, Streete, Co., Westmeath, 1893.

† Read before the Royal Socity, London. Communicated by the author.

zontally into the Observatory window; an achromatic lens enlarges the solar image which is formed on a screen in the room to 4 ft. in diameter.

Behind this screen, and standing on a pier of concrete, is mounted the radio-micrometer. The aperture through which radiant heat reaches the sensitive thermo-couple is a round hole drilled through a thick sheet of brass, and is only 1 mm. in diameter. A white card-board screen is placed in front of the brass one to cut off heat from falling on the latter, and is provided with a hole slightly larger. A beam of lime light is thrown on the mirror of the radio-micrometer, and reflected on to the scale in the usual way. The diagonal mirror of the heliostat is provided with slow motions in two directions, which are moved by long rods and hook joints inside the observatory. Thus any part of the Sun's disc can be placed on the small aperture of the radio-micrometer, and the driving clock will then keep it there.

The observations are taken in the following manner. A small screen is placed over the aperture of the radio-micrometer, and the zero position of the spot of light on the scale noted. The screen is then removed, and the umbra of a Sun spot placed on the aperture. The reading is then taken and entered in column *u*. The image is then moved, so that a part in the neighborhood of the spot, but at *the same distance from the centre of the solar disc*, is placed on the aperture. This reading is entered in column N. Finally, a reading is taken at the centre of the disc, and entered in column C. The throws of the instrument are then got by subtracting the figures in columns *u*, N, and C from the zero. The deflections of the instrument have been experimentally proved to be *strictly proportional* to the amount of radiant heat falling on the thermo-couple. The following is a typical observation taken August 7, 1893, of a large Sun spot then visible. The *umbra* of this spot measured 0.8 in. across on the screen, so that the aperture of the radio-micrometer was only covering about $\frac{1}{400}$ of the area of the umbra.

Zero.	*u*	N.		*u* — *z*	N — *z*
15.8	17.1	20.4		1.3	4.6
15.6	16.9	20.2		1.3	4.6
15.5	16.8	19.9		1.3	4.4
15.3	16.7	19.8		1.4	4.5
15.2	16.6	19.6		1.4	4.4
15.1	16.4	19.5		1.3	4.4
14.9	16.1	19.4		1.2	4.5
			Means	1.31	4.49

The ratio $\frac{\text{umbra of spot}}{\text{neighboring photosphere}} = \frac{1.31}{4.49} = 0.292.$

Five concordant readings gave a mean deflection of 4.57 for the centre of the Sun, which gives for the ratio $\frac{\text{umbra}}{\text{centre}} = 0.287.$

This spot was at a distance from the centre of the disc of about 0.4.

As the radiation from the photosphere falls off from the centre to the edge of the disc, it seemed an interesting point to determine if any change in the ratio of u/C would take place as a spot was carried across the disc by the Sun's rotation. If the spot is, as is generally thought, a depression, the absorption of heat ought to increase as it is carried towards the limb, on account of the increased depth in the solar atmosphere through which the radiation would have to pass. On the other hand, if the spot was floating *above* the absorbing atmosphere the radiation from it would remain constant in any position on the solar disc.

The following is the value of the heat radiation from the photosphere taken along a radius of the Sun, where 0 = centre and 100 the limb. The radiation R equals 100 at the centre.*

D.	R.	D.	R.
0	100.0	60	92.2
10	99.8	70	87.8
20	99.5	75	85.3
25	99.3	80	82.5
30	98.9	90	72.0
40	97.2	95	61.3
50	95.3	98	51.5
		100	42.9

It will be seen by the following observations of spots, taken from August 5 to November 9, that there is distinct evidence that the radiation from the spot does not fall off as rapidly when near the limb as the neighboring photosphere; in fact the ratio u/C remains nearly constant, whereas the ratio u/N gets nearer unity as the spot approaches the limb. The spot observed on the 22d of October is a good example, as the same spot was observed again on the 26th, 29th, and on the 30th, when it had reached within a distance, D, of 95 from the center. It will be seen that on these four dates the ratio u/C was respectively 0.338, 0.360, 0.313, 0.356, whereas the ratio u/N was 0.349, 0.410, 0.706, 0.783.

* "The Absorption of Heat in the Solar Atmosphere," by W. E. Wilson and A. A. Rambaut, 'Proceedings of the Royal Irish Academy,' 3rd series, vol. 2, No. 2 Monthly Notices, vol. 37, No. 1.

	Date.	$\frac{u}{C}$	$\frac{u}{D}$	D
1893.	Aug. 5...........	0.370	0.427	60
	7...........	0.287	0.292	40
	8...........	0.286	0.323	50
	8...........	0.339	0.377	40
	8...........	0.418	0.512	90
	14...........	0.364	0.373	50
	19...........	0.368	0.375	50
	Sept. 2...........	0.309	0.309	10
	3...........	0.298	0.298	10
	4...........	0.420	0.450	30
	4...........	0.430	0.446	30
	7...........	0.287	0.355	85
	Oct. 1...........	0.398	0.401	30
	1...........	0.489	0.570	80
	22...........	0.338	0.349	52
	26...........	0.360	0.410	40
	29...........	0.313	0.706	90
	30...........	0.356	0.783	95
	Nov. 8...........	0.365	0.800	97
	9...........	0.330	0.848	85

Langley,† in 1874 and 1875, measured the radiation from the sun spots. He used a thermo-pile and galvanometer, and obtained as the mean of his results a ratio of 0·54 ± 0·05.

His method was first to take a reading in the neighbourhood of the spot, but between it and the centre of the disc. He then took a reading in the umbra, and, finally, a third reading in the neighbourhood between the spot and the edge of the sun.

The mean of the two photospheric readings he used as a divisor for the umbral reading. He then says, "The decrement of heat as we approach the limb is, though not exactly, yet so very nearly, in the same ratio for photosphere and spots, that no correction is needed on this account for the present observations."

If Langley failed, through want of instrumental means, to notice the difference between the absorption in a spot and the photosphere near the limb, his method would make his umbral readings too high. The mean of twenty observations here equals 0.356, against Langley's 0.54. This is a serious difference, and, I think, can only be accounted for either by the use of superior instrumental means, or by a possible variation in the radiation of spots in different years of the Sun-spot cycle.

It is difficult to see how *too low* a value for umbral radiation could be got, whereas too high a one might be found by want of definition and trembling in the image, so that some of the penumbral radiation would reach the thermo-couple.

EXPERIMENTAL INVESTIGATIONS ON THE EFFECTIVE TEMPERATURE OF THE SUN, MADE AT DARAMONA, STREETE, CO. WESTMEATH.

W. E. WILSON, M. R. I. A., AND P. L. GRAY, B. Sc.

The only tolerably complete series of investigations on this subject up to the present time have been those of Rossetti and Le Châtelier. The results given by other writers have depended more or less on guesses relative to the law connecting radiation and temperature, and differences on this point alone have given values varying between 1500° and 3,000,000° to 5,000,000° C.

Rossetti worked with a thermo-pile exposed directly to the heat of the Sun; the law connecting the deflections of the galvanometer with the temperature of an artificial source of heat having been obtained up to a temperature of *about* 2000° C., from the deflection produced by the heat of the Sun the solar temperature was calculated by extra-polation.

Le Châtelier worked on an entirely different principle, measuring the intensity of the light transmitted through a certain piece of red glass, first from sources at known temperatures up to 1700° or 1800°, and, secondly, from the Sun, the temperature of which was then obtained, as in Rossetti's case, by a process of extra-polation, which is, of course, necessary in any method, until we can raise substanees to a temperature actually as high as that of the Sun, an experiment at present impossible.

Rossetti obtained finally a temperature of 10,000° C., approximately, while Le Châtelier gives 7600° (± 1000°) as the mean of his results. In the paper the difference between Rossetti's result and our own (6200° C.) is discussed, and a possible explanation given.

The method adopted by the authors is a zero method, and the essential point is the *balancing* of the heat from the Sun with that from a platinum strip heated to a high known temperature.

The artificial source of heat was a modified form of Joly's meldometer, the calibration of which can be performed with a very high degree of accuracy. The "radiation balance" is a form of Boys's radio-micrometer containing a duplex circuit, so designed that the heat from the Sun can be made to exert a turning moment in the opposite direction to that due to the artificial source of heat, and by making the apparent area of the latter sufficient-

* Abstract of paper read before the Royal Society, London. Communicated by the authors.

ly great, the radiation from it may be increased so far as to equal that arriving at the radio-micrometer from the Sun.

The following points are considered, after descriptions of the method and apparatus have been given:

1. The law connecting radiation and temperature.

   This is probably the most important factor in the value of the final result. Numerous investigations on the point have been made, which are referred to in the paper.

   After a careful series of experiments we have come to the conclusion that (at least for bright platinum) Stefan's "law of the 4th power" holds, *i. e.*, that for high temperatures (say over 600° or 700° C.) if R = the radiation from a source whose absolute temperature is T, then

   $$R \propto T^4,$$

   a result not wanting confirmation both experimental and theoretical.

2. The emissive power of platinum at high temperatures compared with that of lamp-black.

   On this point the value obtained by Rossetti was used, some considerations being given in support of his figures.

3. The amount of the atmospheric absorption.

   This is fully discussed, and again the value obtained by Rossetti is used.

   Langley's theoretical value for percentage absorption of radiation from a body in the zenith, *viz.*, 41 per cent, is shown to be possibly too great; Rossetti obtained 29 per cent, which appears to be the value best supported by experiment.

   The climate of Ireland entirely prevents a systematic series of investigations on this particular point.

Several subsidiary questions are also discussed, and, finally, the results of about sixty-nine observations are given, which lead to a final mean result for the *effective* solar temperature of 6200° C.

It is pointed out, in conclusion, that the method would probably give excellent results if adopted in some country in or near the tropics, where atmospheric conditions can be trusted to remain more constant for some weeks, or even days, together, and where a series of observations taken at the same part of the year throughout the period of a sun-spot cycle might be hoped to settle the question of how (or if) the solar temperature varies during this time, as any error in the absolute value obtained may probably be considered constant, so that comparative values from year to year might be trusted to indicate any change.

SPECTRA OF THE GREAT NEBULA IN ORION AND OTHER WELL-KNOWN NEBULÆ.*

W. W. CAMPBELL.

A careful study of the new star in *Auriga*, made after its rediscovery in August, 1892, at *Lick* Observatory, led me to the conclusion that its spectrum was nebular. In order to establish that conclusion upon a perfectly firm basis, I investigated the spectrum of the *Nova* and of several well-known gaseous nebulæ as thoroughly as our instrumental means would permit. The observations of the nebulæ were made principally in September and October, 1892, and July, 1893, and lists of nearly all the bright lines observed were published in several journals,† without comment, simply for their bearing upon the questions relating to the new star's spectrum. The investigations were continued on several nights in Sept. and Oct. 1893, and on occasional nights during the past winter, for the information which might be gained concerning the nebular spectrum itself. All the observations of the nebulæ are brought together in the present paper, and discussed to a very limited extent.

Unless otherwise stated, the observations were made with the 36-inch equatorial and the large Brashear spectroscope, using a 21-inch collimator, a dense 60° prism, 10½-inch view telescope and magnifying power of 13 in visual work, and a 10½-inch camera in photographic work. It is unnecessary to say that this spectroscope is satisfactory and efficient when used visually. We shall now show that, theoretically, it is also very efficient in photographing bright-line spectra of large objects. The breadth and length of a monochromatic bright-lined image formed on the photographic plate are substantially independent of the dispersing medium used (prisms or gratings); and for any given width of slit, vary directly as the focal length of the camera lens. The intensity of the bright-line image upon the sensitive plate will therefore vary inversely as the square of the length of the camera. If the collimator and camera are of equal lengths, as is usually the case, the image of any bright-line *on the plate* and the image of the same bright line *in the slit* will be equal to each other in size and intensity,—neglecting loss by absorption, etc. But if the camera is only half the length of the collimator the *area* of the

* Communicated by the author.

* Especially in *Publication* A. S. P. for Dec., 1892, and Sept., 1893; ASTRONOMY AND ASTRO-PHYSICS for October, 1892, and October, 1893; and *Astronomische Nachrichten*, Nos. 3133 and 3189.

photographic image will be decreased four-fold, and its *intensity* increased four-fold. If the camera is only one-fourth the length of the collimator, the intensity will be increased sixteen-fold. The advantage of using a short camera and long collimator for recording faint lines is therefore very great. The principle applies effectively to the study of all large objects yielding bright-line spectra: comets,* large nebulæ, aurora borealis, etc. It is especially applicable to the photography of faint lines in the planetary nebulæ in case the focal-length of the telescope is long: since in that case the images of the nebulæ on the slit-plate are of considerable size, a wide slit can be used, and the reduced and intensified images on the photographic plate are still sufficiently large.

The scale of the photograph is reduced, it is true, by reducing the length of the camera; but the loss can be made up in part by using a denser prism, or entirely by using more prisms. I was able to use a very dense prism to advantage; for though it absorbed ultra-violet light considerably, chromatic aberration prevents me from photographing beyond Hε, except in very large objects.

It will be seen that a three hours' exposure with the spectroscope described above, using the 10½-inch camera, would, for recording faint bright lines in large objects, be equivalent to an exposure of at least 12 hours with the same telescope, and any spectroscope whose camera and collimator were of equal lengths.

The ratio of focal length to aperture is unusually large in the 36-inch equatorial, being 19 : 1. We may assume 15 : 1 as the average value of that ratio for refractors now in use. In the matter of the intensity of nebular images on the slit plates, the average refractor would have an advantage over the Lick refractor of

$$\frac{1}{(15)^2} : \frac{1}{(19)^2},$$

or as 1.62 : 1. If now the average refractor be equipped with a spectroscope whose collimator and camera are equal in length, and the Lick spectroscope's camera be only half as long as its collimater, the advantage of the Lick apparatus over the other in recording faint nebular lines, becomes as 2.49 : 1. In one photograph I used a camera only 5¼ inches long; *i. e.*, one fourth of the focal length of the collimater, which would have increased the ef-

* The use of the short camera in photographing the spectrum of *Comet b* 1893 enabled me to record 25 bright lines with an exposure of about an hour. If the collimator and camera had been of equal lengths, an equivalent exposure would have been at least four hours, which was not possible. I could have secured probably *not more* than six lines.

ficiency at least four-fold more; but the short-focus lens was almost wholly uncorrected and gave poor definition.

Practically the spectroscope is not so efficient. It was designed wholly for visual observations, and when used photographically the effects of flexure enter so seriously as to limit the completeness of the work in many directions. The shifting of the images during a long exposure prevents the accurate measurement of wave-lengths, spoils the definition, and to some extent prevents the recording of the faintest lines. Exposures on the same plate during two or more nights can not safely be made.

I have several times measured the wave-lengths of the principal visual line in the Orion Nebula, G. C. 4373, G. C. 4390 and N. G. C. 7027; but only for the purpose of testing the adjustments of the spectroscope. In no case, I believe, did the resulting velocities in the line of sight differ more than two miles per second from the splendid results obtained by Dr. Keeler in 1890. Further than this, I have made no measures of the nebulæ with high dispersion.

The Great Nebula in Orion.

Visual Observations.

The spectrum of this remarkable nebula has engaged the attention of a great many observers. A history of their work would be interesting and valuable: but it were better written by others; and lack of space here prevents all references to former observations, except those bearing upon what we may regard, up to the present at least, as unsettled problems.

The visual spectrum has been observed by Dr. Huggins and Professor Vogel for the purpose of determining whether the relative intensities of the bright lines vary in different parts of the nebula. Their results, slightly different, are best given by the following quotations, which form an essentially complete history of the subject.

1. "I have suspected that the relative brightness of this line ($H\beta$) varies slightly in different parts of this nebula. It may be estimated perhaps in the Nebula of Orion at about the brightness of the second line (496). The second line suffers in apparent brilliancy from its nearness to the brightest line (501), and may, without due regard to this circumstance, be estimated as brighter than the third ($H\beta$) line." Huggins, in *Phil. Transactions*, 1868, p. 545.

2. "An investigation of the different parts of the nebula gave the results that the three lines (501, 496, Hβ) were everywhere present and that their relative intensities remained always constant."—Vogel in *Astr. Nach.*, No. 1864, 1871, August.

3. "The brightness of these lines (Hβ and Hγ) relatively to the first (501) and second (496) lines varies considerably in different nebulæ; and I suspect that they may also vary in the same nebula at different times, and even in different parts of the same nebula; but at present I have not sufficient evidence on these points." —Huggins, in *Proc. Roy. Soc.*, 1872, May.

4. " . . . In the visible region there is no known alteration of the spectrum of the four bright lines, except, it may be, some small differences of relative brilliancy of the lines."—Huggins, in *Proc. Roy. Soc.*, vol. 46, 1889.

From the foregoing observations, astronomers have generally held the opinion recorded by Miss Clerke, that there is a "fundamental sameness of the visible spectrum of the nebula throughought its entire extent."—*Observatory*, 1889, p. 368; *System of the Stars*, p. 80.

My observations, made Oct. 17 and 18, 1893, lead to a very different conclusion; for I found that the relative intensities of the three lines at wave-lengths 5007, 4959, 4861, which constitute the principal part of the visible spectrum vary within wide limits as the slit of the spectroscope is moved over the different parts of the nebula. For the brightest parts of the nebula, in the vicinity of the trapezium, the relative intensities of these lines are approximately as 4 : 1 : 1. But many of the regions of medium brightness give a spectrum in which the first and third lines are about equally intense; while for many of the faint portions, especially those on the south and west borders of the nebula, the third line is brighter than the first. The isolated portion northeast of the Trapezium surrounding the star Bond No. 734, yields a spectrum in which the third line is much stronger than the first; indeed for some parts of it, the third line is at least five times as intense as the first. It sometimes happens that of two adjacent portions of the nebula in the slit at the same time the first line is stronger than the third for one part, and the third line is stronger than the first for the other part.

The ratio of the intensities of the first and second lines appears to remain practically constant at 4 : 1. The second line is much fainter than the third in nearly all parts of the nebula. In general the Hβ hydrogen line is relatively very strong in the faint outlying regions. It is relatively stronger even in the bright region

around the Trapezium than in any other nebula I have examined except possibly the planetary nebula S. D. M. 12° 1172. As Dr. Huggins has pointed out, "the second line suffers in apparent brilliancy from its nearness to the brightest line." With low dispersion it seems considerably fainter than the Hβ line, even in the vicinity of the trapezium. But when the very bright first line is covered with a heavy micrometer wire, the second line is seen to be fully as bright as the third. This point was further tested by using two gratings, in the first, second and third orders, with which the second and third lines become very faint and widely separated. By narrowing the slit until the lines were rendered almost invisible, the second line was seen with certainty to be a very little brighter than the third line: but of course this result holds true only for the densest parts of the nebula.

The ease with which these observations can be made is due in part to the fact that the image on the slit-plate is large.

The widths of the three brightest lines were carefully examined, as far as possible in all parts of the nebula, to see if any portion gave lines not truly monochromatic. A 60° prism and the first four orders of gratings were used. No variation in their breadths were detected. The lines everywhere appear to be truly monochromatic images of the slit.

The continuous spectrum can be seen in all the fairly bright parts of the nebula. It seems to be at least as strong, relatively, in the faint portions as in the bright ones. I obtained the impression that it is relatively strong in the faint nebulosity surrounding the star Bond No. 734.

In addition to the three prominent lines and the easily observable Hγ line, all of which were discovered by Dr. Huggins, the lines discovered by Copeland* at D_3 and λ 4476 are visible in the brightest part of the nebula. Two measures of the D_3 line in October, 1893, gave 5874 as its wave-length. I have not been able to see the lines observed by Taylor† at λ 5592, λ 5200, λ 4703.

On several nights in October, 1893, I carefully examined the spectra of the principal stars in the nebula for bright and dark lines. These stars are Struve's Trapezium stars A, B, C, D, and Bond's Nos. 685, 708, 734, 741. No bright lines were seen in any of them. Special care was taken in observing stars C and No. 685 (Flamsteed's ϑ_1 and ϑ_2), but I was unable to verify Espin's‡ 1890 observations that they contain bright lines. Hβ

* *Monthly Notices*, R. A. S., vol. 48, pp. 360-2.
† *Monthly Notices*, R. A. S., vol. 49, pp. 124-6.
‡ *Astronomische Nachrichten*, No. 2863.

was observed to be dark in No. 685, and probably also in star C. Hβ was observed to be dark in No. 734. No other dark lines were seen with certainty in any of the eight stars. The points of intersection of the stellar spectra with the bright nebular lines were especially examined for bright and dark lines. In every case it was judged that the stellar spectra were strictly continuous at λ 5007 and λ 4959, and contained dark Hβ lines, for the following reason: The nebular lines in the immediate vicinity of the Trapezium are neither brighter nor fainter, perceptibly, than they are just to either side of it; so that if we pass the slit *between* and *not including* those stars, the spectral lines furnish no clue that those stars are very near. The intersections of the stellar spectra with the bright lines λ 5007 and λ 4959 are certainly slightly brighter than the continuous spectra of the stars just to one side of the points of intersection, as would be the case if at the intersections the bright nebular lines and continuous star spectra are superposed. At the Hβ intersections there seemed to be little or no increase of brightness, as would be the case if the Hβ star lines are dark. But all visual observations of this kind may be more or less uncertain; and the photographic observations, referred to on a later page, are more decisive. The visual observations are made best with gratings.

Photographic Observations.

One very successful photograph of the spectrum was obtained on Oct. 11, 1892. The photographic field was terminated at λ 383 by the narrow camera tube. Sixteen prominent lines were recorded between λ 501 and λ 383, besides some very faint ones. A list of 18 of these lines was printed in A. AND A.-P. for October, 1893, pp. 723-4, with no discussion beyond the statement that the photograph presented almost no points of resemblance to Dr. and Mrs. Huggins' 1888 photograph of the same region. I further said that 12 of the 18 lines were "not previously observed." The number *twelve* is not correct; for I have since found that Lockyer* and Fowler obtained five spectoscopic negatives of this nebula on Feb. 2, 8, 9, 10, 11, 1890, with which my negative appears to be more or less identical. I now take pleasure in crediting them with the discovery of some of those twelve lines, and regret the temporary injustice done them. Likewise another one of the twelve, the hydrogen line λ 3836, was observed by Dr. and Mrs. Huggins,† whose photographs were made on March 14, 15,

* Lockyer's paper, read before the Royal Society on Feb. 13, 1890, is in *Proc. Roy. Soc.*, Vol. 48, pp. 199-201.

† Dr. and Mrs. Huggins' paper, communicated to the Royal Society on April 16, 1890, is in *Proc. Roy. Soc.*, Vol. 48, pp. 213-216.

17, 1890, appear to be substantially identical with those obtained by Lockyer and Fowler. Both these very important series of negatives are as yet incompletely described, and we cannot establish the identity of the two series with each other, nor with my photograph, though they all seem to agree in their general character.

These observations were made early in 1890, before I became specially interested in spectroscopic work, and probably too late to be included in the text or referred to in the literature-index of Scheiner's *Spectralanalyse der Gestirne* (issued late in 1890). I had searched all the technical astronomical journals issued subsequent to the season of 1890 for late observations without success. I regret having temporarily overlooked them.

I resumed photography of the spectrum in September, 1893, as soon as the nebula came into an accessible position. A list of the principal negatives follows.

1893 Sept. 12. A three hours' exposure with a spectroscope* attached to the 12-inch equatorial showed about twenty bright lines between λ 501 and λ 372, and traces of a few others. A list of these lines will be given further on. The slit was about 0.42 inch long. It was placed east and west, across the trapezium. It included not only the dense central region of the nebula, but also some of the fainter portions. All the prominent hydrogen lines extend into the fainter regions apparently with *relative* intensities unchanged; while the intensities of the lines λ 501 and λ 496 fall off more rapidly, and the intensity of the line λ 373 less rapidly, than in the case of the hydrogen lines. The latter is due at least in part to chromatic aberration, and it may also be partly real; for Gothard† has found that the line 373 "is always very intense in the large irregular nebulæ, is always very faint in the true planetary nebulæ."

1893 Sept. 17. Another exposure with the 12-inch was made, but with the slit somewhat differently situated, for testing the relative intensities of the brightest lines. The results are the same as those obtained Sept. 12.

1893 Sept. 18. A three hours' exposure was made with the 36-inch and 10½-inch camera, with the slit placed upon the brightest region southwest of the Trapezium, but not including the

* This spectroscope was constructed for me by the Observatory carpenter, all but the slit and optical parts being of wood. It gave splendid definition, and for long exposure showed no signs of flexure. It weighs about 10 pounds. Next winter I hope to have more efficient optical parts similarly mounted for work on the same nebula.

† ASTRONOMY AND ASTRO-PHYSICS, January, 1893, p. 55.

Trapezium, for the purpose of detecting any possible variations in brightness of the hydrogen lines. The photograph seems to be identical in every way with those of the Trapezium region, except as they are affected by chromatic aberration.

I made several comparatively short exposures, during September and October, on the different bright regions in the nebula, in order to see if I could confirm Dr. Huggins' earlier results showing remarkable absences of certain hydrogen lines. My photographs show no changes in the relative intensities of the hydrogen lines. $H\delta$ is always present, and not much fainter than $H\gamma$.

1893 Oct. 12. A four hours' exposure was made with the 36-inch, using a 5¼-inch camera with the 21-inch collimator. The slit included and preceded the Trapezium. The negative shows fully twenty-five bright lines, with traces of others, between λ 501 and λ 372. The cheap 5¼-inch lens employed is not large enough to include the whole beam of light, is poorly corrected around the edge, and necessarily gives poor definition. For that reason no further use was made of it.

1893 Nov. 10. An exposure on the isolated nebulosity surrounding star Bond No. 734 recorded the hydrogen lines $H\beta$, $H\gamma$, $H\delta$ and $H\varepsilon$ and no other bright lines; thus confirming the visual observation given above, of the great relative intensity of the hydrogen lines in this region.

All the long exposures on the condensed parts record the continuous spectrum of the nebula.

The wave-lengths of the bright lines shown with certainty on my principal negatives are tabulated in Table I. The relative intensities are assigned from the negatives taken with the 36-inch which on account of chromatic aberration, etc., are considerably different from those taken with the 12-inch, which in turn would be different from those obtained with a reflector.

Very little can be done in the way of assigning the chemical origin of these bright lines. Nine are due to hydrogen, but the sources of the others are unknown. The line at λ 5874 is probably D_3. The line at λ 4472 is possibly identical with a line always present in our Sun's chromosphere. The others can at present be classed only as nebular lines.

In reference to the *forms* of the bright lines shown on their 1890 (and also their 1888) photographs, Dr. and Mrs. Huggins write: " . . . it is significant that the hydrogen lines are sensibly stronger and broader on the plate as the Trapezium with its stars is approached. . . .

TABLE I.—BRIGHT LINES PHOTOGRAPHED IN THE ORION NEBULA.

1892. Oct. 11	1893. Sept. 12.	1893. Sept. 18	1893. Oct. 12.	1893. Nov. 16.	Description.
5007	5007	5007	5007		1st nebular line, very bright.
4959	4959	4959	4959		2d " " " "
4861	4861	4861	4861	4861	Hβ very bright.
4713			4716		Bright.
4661			4664		"
4473	4472	4472	4472		Very Bright.
4390	4389	4389	4389		Bright.
4363	4364	4364	4364		"
4341	4341	4341	4341	4341	Hγ, brightest line in spectrum.
4265			4265		Very faint.
423			423		Extremely faint.
4145	4143	4143	4143		Faint.
4122		4121	4121		"
4102	4102	4102	4102	4102	Hδ, very bright.
4067	4067	4067	4067		Bright.
4026	4026	4026	4026		"
3969	3969	3969	3969	3969	Hε, very bright.
3889	3889	3889	3889		Hζ, bright.
3868	3869	3869	3869		Bright.
3836	3835	3835	3835		Hη, bright.
	3798	3798	3798		Hϑ, faint.
	3770		3770		Hι, "
	3749		3749		Hκ, very faint.
	3727	3727	3727		Very bright.

"The lines of the new (1890) photographs contain two very strong and abruptly bounded blotches, and a third one less marked.

"These brighter blotches, corresponding to different conditions of closely adjacent nebular matter, give an explanation of an appearance which we recorded last year in speaking of the strong line 'about λ 3724' [on the 1888 negative]. 'On one side of the star spectra [two Trapezium stars] this line is a little broader than on the other side; but as a similar appearance is presented by Hγ and the stronger lines of the group it may arise from some optical or photographic cause.'*

"We now learn that this difference between two parts of the lines indicates probably a different condition of the nebula on the two sides of the star-spectra."†

Some of my negatives taken with the 36-inch show analogous variations in the widths of the lines, but I do not consider that the observed variations are real. There are several reasons for this belief:

(*a*).—The narrowest part of the bright line must be at least as

* *Proc. Roy. Soc.*, vol. 46, pp. 54-5.
† *Proc. Roy. Soc.*, vol. 48, pp. 214-216.

wide as the slit, if the camera and collimater are equal in length. Any broader part of the line must be wider than the slit, in which case it is really a band made up of light not monochromatic. If photographs obtained with the low dispersion of one or two 60° prisms show the increased breadth of parts of the lines, a grating giving ten or twelve times as much dispersion and a high-power eye-piece should readily show the non-monochromatic character of the lines, and their varying widths in different parts of the nebula. My visual observations, described above, do not show any such conditions.

(*b*).—My photographs made with the 12-inch telescope and the very rigid spectroscope show lines 0.42 inch long *of constant width and all the lines are of equal width*. The slit was long enough to include a wide region each side of the Trapezium.

(*c*).—On those negatives which show lines not of uniform width, the *general* form of the images is the same for all the lines prominent enough to photograph in both bright and faint parts of the nebula. The *general* form is the same whether the slit crossed the Trapezium or was directed to some other region;—other things being equal the middle of the slit gave a slightly greater width to the lines than the ends did;—therefore the increased breadth is due in part to instrumental causes.

(*d*).—The lines are slightly broader for bright parts of the nebula crossed by the slit than for adjacent faint parts, for two reasons: first, the spreading of the over exposed images of the bright parts; secondly, and chiefly there is flexure in the spectroscope during the exposure. The effect of flexure is to broaden the brighter parts of the line more than the fainter parts; for the brightest parts photograph in their first and last positions on the plate, whereas the faintest parts photograph only where the successive positions of the images overlap each other.

In this connection we should not lose sight of a vigorous discussion carried on in 1890, regarding the character of the chief line in the Nebula of Orion. From their published paper* I understand Dr. and Mrs. Huggins' views [confirmed by the observations of Young, Copeland, Keeler, and others] to be that the principal line in this spectrum is sharply defined, very narrow under the highest dispersions, and perfectly monochromatic. My photographs show the principal nebular line (λ 5007) to have always the same form as the $H\beta$, and $H\gamma$ and other prominent lines, just as Dr. and Mrs. Huggins' negatives show that the hydrogen and other strong lines are similarly broadened. Now if the

Proc. Roy. Soc., vol. 48, pp. 207-212.

principal line was shown by visual observations in 1890 to be sharp, narrow and monochromatic, it seems to me that to accept the photographic irregularly broadened form of the images as their real form, is to ascribe to the principal line a very different character.

I have photographed the spectra of the six principal stars in the central part of the nebula. They are Struve's Trapezium stars A, B, C, D, and Bond's stars Nos. 685, 734. The photographs were made principally on Oct. 18, Nov. 10, Dec. 15, 1893 and Jan. 11, 12 and 23, 1894. So far as I know these spectra have never been photographed with a slit before, except in the case of two of the Trapezium stars by Dr. and Mrs. Huggins in 1888. My photographs show continuous spectra, numerous dark lines, and no bright lines; and all the spectra conform closely to the "Orion type;" though some of the dark lines are broader than we would expect to find in that type. Only one part of the spectrum of the faintest Trapezium star, B, was photographed, on account of its faintness. The Hδ line is dark, there are no bright lines in that vicinity, and its spectrum is probably very similar to the others. Two long exposures on the nebular spectrum show the combined spectrum of the Trapezium stars up to H$\varkappa$, and the hydrogen lines Hζ, Hη, Hϑ, Hι are seen to be dark. They may safely be attributed to the brightest star C. All the dark lines photographed in the six stars are recorded in the first six columns of Table II. On many of the negatives, some of the bright nebular lines were necessarily recorded at the same time; but they are in general considerably narrower than the dark star lines, and pass through the centers of the dark lines. The dark Hβ lines in the stars C, D, A, are scarcely wider than the Hβ bright nebular line, but there is no doubt they are dark. The points of intersection of the nebular lines λ5007 and λ4959 with the spectra of the stars C, D, A are brighter than the continuous spectra each side of the points of intersection, owing to the superposition, probably, of the bright nebular lines and continuous star spectra. It is possible, to be sure, that the increased brightness at those points could be due to bright lines in the stars, and such may be the case; but taking the other parts of these spectra and other spectra of this type into consideration, there seems to be no necessity for such an assumption.

Our knowledge of the spectra of β, δ, and ε Orionis is very fragmentary, and I have also made a few photographs of them. Nearly all the lines observed in δ Orionis between λ 493 and λ 393 are tabulated in column seven below. β and ε Orionis contain a

great many lines, and I have tabulated in columns eight and nine only those which match lines observed in the six faint stars; except that in these cases I photographed with isochromatic plates, also, and detected a prominent dark line in the position of the sodium D_{12}, and a very prominent dark line in the position of the helium D_3. The latter is probably D_3; in which case it is the first time, so far as I know, that a dark D_3 has been observed. A great many prominent lines in β and ε Orionis and a few in δ Orionis have been omitted.

TABLE II.—DARK LINES PHOTOGRAPHED IN ORION STARS.

ΣC	ΣD	ΣA	ΣB	No. 685	No. 734	δ Orion.	ε Orion.	β Orion.
							5893	5893
							5876	5876
4924	4924	4824		4924		4924	4924	4924
4861	4861	4861		4861	4861	4861	4861	4861
				4715		4715	4715	
4688				4688		4688	4688	
						4662	4662	
	4652	4652		4652		4652	4652	
461								
454						454		
4472	4472	4472		4472	4472	4472	4472	4472
4389	4389	4389		4389	4389	4389	4389	4389
4341	4341	4341		4341	4341	4341	4341	4341
								4267
								4230
4203						4203		
	4143	4143			4143	4143	4143	4143
	4121	4121		4121	4121	4121	4121	4121
4102	4102	4102	4102	4102	4102	4102	4102	4102
	4067	4067				4067	4067	
4026	4026	4026		4026	4067	4026	4026	4026
3969	3969	3969		3969	3969	3969	3969	3969

A comparison of these dark lines with the bright lines in the nebula points to a most interesting result.

In 1890 Dr. Scheiner called attention* to a dark line λ 4471.4 discovered by him in β, γ, δ, ε, and ζ Orionis (and in β Persei), stating that it probably coincided with Copeland's very faint bright line λ 4476 in the Orion nebula, and that it was evidence of the physical connection of the nebula and the five bright Orion stars mentioned. Professor Pickering† in 1891 called attention to a further coincidence of the dark line λ 470 (probably the line observed by me at λ 4688) in the bright stars in Orion and a bright line λ 470 in the planetary nebulæ.

* *Sitzungsberichte d. Berlin. Akad.*, 1890, p. 145.
† *Astronomische Nachrichten*, No. 3025.

To facilitate further comparisons, I have placed in the first column of Table III the bright lines observed by me in the Orion Nebula, in the second column the dark lines in the faint stars in the nebula, and in the third column the corresponding lines in the bright stars β, δ and ε Orionis.

TABLE III.—COMPARISON OF BRIGHT NEBULAR AND DARK STAR LINES.

Nebular bright lines.	Faint-star dark lines	Bright-star dark lines	Remarks.
		5893	D_{12}. Faint stars not photo'd in yellow.
5874		5876	D_3.
5007			Stellar spectra apparently continuous.
4959			" " " "
	4924	4924	Bright line suspected in Orion Neb. & G. C. 4390.
4861	4861	4861	$H\beta$.
4714	4715	4715	Observed in several nebulæ.
	4688	4688	" " five "
4662		4662	" " a few "
	4652	4652	Not observed in any nebulæ.
	461		Probably observed in G. C. 4390.
	454	454	Not observed in any nebulæ.
4472	4472	4472	Very common in nebulæ.
4389	4389	4389	Observed in several nebulæ.
4364			Very common in nebulæ.
4341	4341	4341	$H\gamma$
4265		4267	Observed also in G. C. 4390.
423		4230	Not observed in other nebulæ.
4143	4143	4143	Observed also in G. C. 4390.
4121	4121	4121	" " " " "
4102	4102	4102	$H\delta$
4067	4067	4067	Observed in several nebulæ.
4026	4026	4026	" " " "
3969	3969	3969	$H\varepsilon$
3889	3889		$H\zeta$
3869			Evidently very common in nebulæ.
3836	3836		$H\eta$
3798	3798		$H\vartheta$
3770	3770		$H\iota$
3749			$H\varkappa$
3727			Evidently very common in nebulæ.

We know the ultra-violet hydrogen series $H\zeta$ to $H\varkappa$ is dark in the prominent Orion stars; and (counting the hydrogen lines) we can say that of the 25 known bright lines in this nebula, given in the above table, at least 19 are definitely matched by dark lines in the Orion stars and at least 15 by dark lines in the six faint stars situated in the dense parts of the nebula. Further, nearly all the dark lines in the faint stars are matched by bright lines in the nebula. In general, the prominent dark lines correspond to prominent bright lines, though there are

some exceptions. There is no reason known at present for supposing that the prominent nebular lines λ 5007, λ 4959, λ 4364, λ 3869, λ 3727 are matched by dark stellar lines. They appear to remain characteristic of the nebular spectrum, and of no other type. If these dark-line stars are *within* or *beyond* the great nebula, and the stellar light were absorbed by the nebula through which it passes, we would expect dark stellar lines in the positions of these five prominent nebular lines. The fact that some or all of these positions are not occupied by dark lines renders it doubtful whether any of the lines are due to absorption by the nebula proper, and we cannot safely say that these stars are beyond the nebula or are physically connected with it. We can safely say, however, that they are closely related to the nebula in chemical constitution and *relatively* closely in physical condition.

Dr. and Mrs. Huggins' 1888 photograph * of the spectra of this nebula and two superposed trapezium stars is well-known to all readers of spectroscopic literature. Several most interesting and important conclusions have been based upon it. Nevertheless, I have been unable to verify many of their observed facts; and as this subject is of superlative importance,—situated, as it is, at or near the beginning of every system of stellar classification,—it is desirable to point out definitely the differences in our results. I shall limit the comparison to the region λ 420—λ 397, since my photographs of the stellar spectra do not extend beyond Hε.

Dr. and Mrs. Huggins obtained:	Mr. Campbell obtained:
1. No trace of bright Hδ in nebula.	1. Very prominent bright Hδ in nebula.
2. " " " " λ 4067 " "	2. Prominent bright λ 4067 in nebula.
3. " " " " λ 4026 " "	3. " bright λ 4026 in nebula
4. " " " " Hε " "	4. Very prominent bright Hε in nebula.
5. No trace of dark lines at Hδ, λ 4067, λ 4026, Hε in Trapezium stars.	5. Very prominent dark lines at Hδ, λ 4026 and Hε, and prominent dark line at λ 4067, in Trapezium stars.
6. A group of six bright lines in Trapezium stars between λ 4167 and λ 4116, possibly also in nebula; no dark lines in stars.	6. No bright lines in stars between λ 4167—λ 4116; bright lines in nebula at λ 4143, and λ 4121; dark lines in stars at λ 4143, λ 4121.

It will be seen that, in this region, I did not succeed in photographing their faint lines, and they did not record my bright lines; I did not obtain their bright lines in the stars and they did not obtain my dark lines in the stars. It is difficult to explain

* Described in *Proc. Roy. Soc.*, Vol. 46, pp. 40-60.

these differences on the basis of variations in the stellar and nebular spectra. But if they are due to real variations, we have here the most remarkable case of variation known in astronomy.

Although the observations described in the preceding pages are more complete and lead to more interesting results than were even hoped for at the beginning of the work, it is evident that they throw very little light upon questions relating to the composition and physical condition of this important nebula. Apparently we cannot reproduce nebular conditions in terrestrial experiments, and every advance in our knowledge of the nebulæ only adds greater emphasis to that point. Nevertheless, it is desirable that the observations of the spectrum should be made as complete as possible, however backward our interpretations of the observed facts may be. Holding to that view, I hope next season, with more efficient spectroscopes, to extend the results considerably further.

(To be Continued).

THE NEW STAR IN NORMA.*

EDWARD C. PICKERING.

On February 28, 1894, Professor S. I. Bailey succeeded in obtaining a photograph of the spectrum of the new star in Norma. The photograph was taken with the Bache telescope, having the 13° prism placed in front of the object-glass, and had an exposure of 166 minutes. Nearly all the light is concentrated in the line $H\gamma$, wave-length 434. Traces of several other lines are perceptible in the contact print sent by Mr. Bailey. Their wave-lengths can probably be determined when the original negative is received in Cambridge. The brightness appears to have diminished about half a magnitude between October 29, 1893 and February 28, 1894. Professor Bailey has looked at Nova Normæ with the 13-inch telescope at Arequipa, employing magnifying powers up to 800, without perceiving any disk.

Harvard College Observatory.
Cambridge, Mass. April 13, 1894.

ASTRO-PHYSICAL NOTES.

All articles and correspondence relating to spectroscopy and other subjects, properly included in Astro-Physics, should be addressed to George E. Hale, Kenwood Observatory of the University of Chicago, Chicago, U. S. A. Authors of papers are requested to refer to last page for information in regard to illustrations, reprint copies, etc.

* Communicated by the author.

The Influence of Slit-Width on the Appearance of the Spectra of Comets.—Professor Kayser's important paper seems to furnish a complete explanation of the anomalies hitherto observed in the spectra of comets. In a general way, the influence of slit-width on the position of the edge of a fluting has been understood and allowed for by spectroscopists, but a detailed consideration of all the consequences attending the use of a wide slit with small dispersion is now given for the first time. Now that photography has taken the place of the difficult eye observations, it is probable that fewer anomalies will be found in cometary spectra, and that the systematic changes in the position of the carbon bands which have in some instances been recorded, are to be ascribed to the varying conditions under which the observations were made.

Dr. Chandler's Criticisms of the Harvard Photometric Observations.—In A. N. 3214, Dr. Chandler calls attention to serious errors in the photometric observations which have been made with the meridian photometer at Harvard College Observatory. This is evidently an important matter, and it should be thoroughly investigated, as one of the chief purposes of the Harvard Photometry is to provide a series of standards which may serve as a basis for future work. The errors referred to were detected by comparing the observations of variable stars with the magnitudes deduced from their known elements. Dr. Chandler says, "In constructing the Second Catalogue of Variable Stars, recently published, I had occasion to examine with some care the photometric observations in Vol. XXIV. of the Harvard College Observatory Annals. It soon became manifest that there were numerous incongruities in the observations there given, with the known characteristics of variation of many of the stars, as well as with other well attested series of contemporaneous observations. To be more specific, a list of some of these contradictions is here given. It is by no means complete, but comprises perhaps the more important results of a somewhat desultory examination, which was only carried far enough to justify, as it seemed to me, an impression of distrust whether any of these observations are suitable for any precise or critical purpose."

A list of important discrepancies is given by Dr. Chandler, one or two of the most striking of which may be given here as examples.

112. R Andromedæ. The 1886 observations are certainly all erroneous by two or three magnitudes, since the minimum occurred 1886 Dec. 5 at 12.8 mag., by the elements; the latter being confirmed by Parkhurst's observations.

4160. T Leonis. The observation 1882 April 17 gives 7.4 mag. This is very surprising, as the star has never been certainly seen by any one except Peters, and by him only on two occasions at 10.11 mag., on all others at 13 mag. or less.

5826. T Scorpii. This is the new star of 1860, in the cluster 80 Messier, 5″ north following its center, in position angle 55°. It appeared suddenly (7.0 mag.) and faded rapidly to invisibility, and has never since been seen, except possibly once on 1869 June 1, when it was uncertainly suspected by Schönfeld. I am perfectly familiar with this cluster and its surroundings, having looked for the missing star more than a hundred times at the very least, unsuccessfully; Schmidt, at least one thousand times between 1860 and 1877. Except on the very improbable hypothesis that the star, by a happy accident, had returned at the very epoch when the photometer happened to be set upon it in 1886, the three observations of it as 8.2 mag. must be mistakes.

Dr. Chandler attributes many of these discrepancies to erroneous identifications of stars with the meridian photometer, for, the mirrors of that instrument being movable through a small range in Right Ascension, there is no such check on the identity of the star as there is in ordinary meridian observations. If this is the case the errors will be comparatively few in number, and may be corrected. If on the other hand they arise from a fundamental weakness of principle or inaccuracy of observation, as certain discrepancies in comparatively bright stars seem to indicate, the value of the whole work becomes practically nothing. In either case Dr. Chandler has done a valuable service in pointing out the existence of these errors, and it is to be noted that all his remarks are strictly within the bounds of legitimate criticism.

A New Triple Achromatic Object-Glass.—At the March meeting of the Royal Astronomical Society, Mr. H. Dennis Taylor described a new and "perfectly achromatic" object-glass which he has for some time been engaged in perfecting. Mr. Taylor explained that by "perfectly achromatic" he meant that the chromatic aberration to be expected in an objective of even two feet aperture is no greater than that which is introduced into a reflector whose focal-length is 7½ times its aperture, when a 1-inch Huyghenian eye-piece is used for viewing the image. The objective consists of three lenses of three different kinds of glass. The outside lens is a hard baryta light flint, and is double convex in form; the inner lens is double concave, of a certain new boro-silicate flint; the back lens of the combination is a positive meniscus nearly plane on the outside surface, and is made of light silicate crown glass. All three varieties of glass are made by Schott, of Jena. The flint glass of the interior lens is slightly liable to tarnish and hence is protected by being enclosed between the hard outer lenses. It is not stated whether the lenses are cemented, but cementing would be impossible in a large lens without great injury to the definition, and without it gradual tarnishing of the interior surfaces would seem to be unavoidable; nevertheless Messrs. Cooke & Sons guarantee the permanence of objectives made on this plan.

Some years ago Professor Hastings made a double objective, using a certain boro-silicate flint and a potassium silicate crown made at Jena, in which the chromatic aberration was reduced to something like five per cent. of its amount in the usual construction, so that the image was practically free from color, but the flint glass was liable to tarnish, while the crown glass was hygroscopic, and was therefore always covered by a film of moisture. Singularly enough the polish of the crown lens does not seem to have been affected by this peculiarity of the glass, and is apparently as good at the present time as it was when the objective was made; but it would seem that *some* action tending to destroy the surface must be going on, nevertheless. The flint glass is nearly identical with that used by Mr. Taylor.

The theory of the triple objective was discussed by Hastings in the *American Journal of Science*, December 1879. Mr. Taylor has a greater variety of glasses from which to select the lenses of his combination, and has doubtless chosen the kinds most suitable for his purpose; otherwise the objective does not seem to differ materially from that proposed by Hastings. A section of Mr. Taylor's objective accompanies the advertisment of Messrs. Cooke in *Knowledge*. If this figure is drawn to scale, the interior lens is extremely thin in the middle, and must be very difficult to work.

An impression seems to have been produced in some quarters that *all* Jena glass is subject to some such drawback as that mentioned above, and therefore it

may be worth while to state that only a few very special varieties are so affected, and that with these exceptions all the glass made at Jena is perfectly durable.

New Star Spectroscope for the Lick Observatory.—A large and very fine star spectroscope for the Lick Observatory has just been completed at the workshops of Mr. J. A. Brashear. We shall hope to give a detailed account of this fine instrument in a future number.

The Aurora of March 30, 1894.—On the night of March 30, an unusually fine display of the aurora was observed in the United States and in Europe. At Alexandria, Va., the streamers when highest extended nearly to the zenith; at Allegheny they extended fifteen degrees south of the zenith, at $9^h\ 50^m$ Eastern Time. Numerous reports have been received, from which we give a few extracts.

"On March 30 at 7:45 P. M. a brilliant aurora borealis was visible here. At first it appeared as a large patch of pale red light in the north east. It faded away, and reappeared suddenly, with streamers reaching up some 45°, and extended around to Polaris; it then faded again, and reappeared in two sections,—bright red patches with dark and light streamers. The two sections seemed disposed to spread out and join each other. The western section at its brightest was about 30° wide, enveloping and almost obliterating the constellation Cassiopeia; the other formed east of Polaris, and extended nearly to Arcturus, streaming upward between Ursa Major and Ursa Minor. It did not become so brilliant again, as long as we watched it, but traces of it could be seen until after midnight."

JOHN W. JOHNSON,
Associate Professor of Physics.

University of Mississippi.
March 31, 1894.

The following is an extract from a letter received from Dr. Veeder:

"You will probably be interested in the results of the observations of the aurora of Friday, March 30, which was well seen, in the vicinity of the 77th meridian especially, from South Carolina northward into Canada, and many reports concerning which are at hand. The peculiar feature is that from Lyons northward into Canada, from 7:40 to 8:38 P. M., it was located south of the zenith exclusively, there being not even the faintest auroral glow toward the north during this hour. In Pennsylvania it was seen north and south of the zenith, and further south it was seen toward the north exclusively, rising 25° above the northern horizon in South Carolina. From this it appears that the center of the luminous mass was at about latitude 40° to 41°, and 600 miles further south its summit was seen toward the north, having the elevation of 25°. This would give approximately a right-angled triangle with a base of 600 miles and an angle at the base of 25°, corresponding to an altitude of the aurora of about 300 to 350 miles at the highest point.

The solar conditions also were characteristic at the time of this aurora, being such as have been described heretofore. There was a disturbance exactly on the eastern limb of the Sun and south of the equator, which is the precise location to have an auroral effect at this season of the year. With the disturbed area north of the equator at this season, increase of thunder storms takes the place of the aurora. Both these points were well illustrated many times during March, as well as in the months preceding. The most noticeable instance showing these relations recently is in connection with a very persistently disturbed area extending north and south of the equator, whose successive reappearances at the limb

have been attended by increase of thunderstorms and auroras. The dates are as follows: May 22, June 18, July 15, Aug. 12, Sept. 8, Oct. 5, Nov. 1, Nov. 29, Dec. 26, Jan. 22, Feb. 18, March 18, or eleven returns in 300 days, giving a period of 27¼ days. There were auroras on all these dates, those on June 18, July 15, Aug. 12 and Nov. 1 being especially fine. In like manner there was increase of thunderstorms on all of them, extending throughout the winter in a way that is most remarkable. Thus buildings were struck by lightning in New York State on Christmas night, an occurrence which is most unusual. The outbreak of vigorous electrical storms and tornadoes on March 18th is fresh in memory. Since October the U. S. Weather Bureau has published in the *Monthly Weather Review* a table showing the numbers of stations reporting auroras and thunderstorms each day, and their geographical distribution. In these tables the dates above indicated stand out prominently.

I am receiving some excellent reports from Siberia, Finland, Sweden, Scotland and England, especially. The Greenland reports will become accessible upon the return of the ship "Falcon," which will visit Mr. Peary's station as soon as removal of the ice will permit." M. A. VEEDER.

Lyons, N. Y., April 11, 1894.

Lowell Observatory.—A private Observatory is to be erected and kept up during the coming summer and autumn by Mr. Percival Lowell in Arizona. His objects are: (a) the study of Mars during its approaching opposition; (b) the determination of the atmospheric conditions most favorable to astronomical observation. He will take with him Prof. W. H. Pickering and Mr. A. E. Douglass.

The observatory's equipment will consist of an 18-inch lens by Brashear, a 12-inch lens by Clark and a six-inch lens by Clark, the two former to be mounted in a dome 34 feet in diameter. The dome is to be built of wood and constructed in parallel arches covered with wire netting and canvas upon a plan devised by Professor W. H. Pickering.

It is hoped to have the telescope in place and the Observatory in working order by June 1st.

The note in our last number relative to this Observatory was founded on a newspaper report: the information given above is from a reliable source.

Glazebrook's Light.*—It is not often that one steps backward in writing a second volume on a subject which he has previously treated. But we cannot avoid the conclusion that this is what Mr. Glazebrook has done in his new volume on light. For in his *Physical Optics*, which appeared more than ten years ago, he gave us a lucid, and to many a student, very helpful, discussion of the nature of light. In the earlier volume the wave surface was emphasized as being the fact of nature; its normal, the so-called ray, as being a mathematical convention for fixing the plane of the wave-surface at any point. Following Fresnel, everything was deduced from the principle of secondary waves together with the principle of interference. Lenses he described as simply instruments for changing the curvature of the incident wave-front. Phenomena of double refraction were, of course, treated by use of the wave-surface.

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* *Light, an Elementary Text-book, Theoretical and Practical*, by R. T. Glazebrook, M. A., F. R. S, (Cambridge: University Press., 1894).

But, in the volume before us, all is changed. Only with difficulty does one believe that the two books are written by the same man. A prism is no longer a device for rotating a wave-surface; a focus is no longer the centre of curvature of a certain emergent wave-surface. Definitions, demonstrations, and figures are all in terms of rays, excellently adapted to elucidate the corpuscular theory.

One has to read but a few pages of the later volume to learn that it is by no means intended as a substitute for the earlier. But, unless its object be completely misunderstood, it is much more than a laboratory guide, it is an exposition of the nature of light; and, as such, most of its pages are devoted to what light is not, namely, rays.

The plea of mathematical simplicity can hardly be urged in its favor: for, though there are few formulæ in the book, what few there are might have been derived more quickly and elegantly by use of the wave-surface.

To be sure, the subject is here purposely treated in an elementary way; but Mr. Glazebrook himself, not to mention Lord Kelvin and Prof. S. P. Thompson, has shown us that in the wave-surface treatment there is an elegant simplicity which makes the subject at once clear and attractive, and which unifies the discussion in a marvelous way, allowing the student to pass from the treatment of lenses to the treatment of mirrors and gratings without the slightest interruption of continuity either of method or of nomenclature.

What is more important still, the method of the wave-surface allows one to begin the subject with the experimental evidence of Young and Fresnel for the wave theory, and then proceed through the whole discussion without any break in method. Two pin holes in a piece of platinum, or two parallel rulings with the point of a knife on an undeveloped photographic dry plate may be used to view a candle flame, or better still an illuminated slit in a visiting card. They furnish the student very cogent evidence for the wave theory. Many of Fresnel's diffraction and interference experiments are easily shown, and make a natural starting point. Lord Kelvin's beautiful wave model is within reach of the most impecunious, and is a powerful aid in clearing up the beginner's ideas of transverse waves.

But it must not be forgotten that the volume under review is intended also to assist the student in the laboratory and it goes almost without saying that it contains many excellently selected experiments.

Our experience is, however, that the student who carries with him, to the laboratory, his ideas in terms of the wave surface has the clearest possible views regarding his experimental work. We are still speaking of the beginner. If his problem be to determine the refractive index of a liquid by its "lifting power," say, he measures the change of curvature impressed upon the spherical wave-front at emergence from the plane surface of the liquid. The adjustment of optical instruments, measurement of focal lengths, and determination of curvatures fall into line at once; so that, in this manner, as well as that employed by Mr. Glazebrook there is no gap between lecture room and laboratory.

And as to some problems, a trifle more advanced, as, for instance, the resolving power of prisms, or the magnifying power of the astronomical telescope, one is continually astonished at the ease with which they yield to treatment in terms of the wave-front, and that without any mathematics other than the elements of geometry.

Mr. Glazebrook's contributions to optics are too well known to need rehearsal here. We all know what he might have done in the way of writing an elementary text-book on light. H. C.

Variation of Gravity with Altitude.—When the new standards of mass were recently received by the United States from the International Bureau, it was remarked by one of our officials that two kilogram masses placed upon one pan of a balance showed a different weight according as they were placed side by side or one on top the other. That their weights *were* different in these two positions no one doubted; for in the second case one kilo was certainly some five or six centimeters farther away from the center of the Earth. But the idea of this difference being detectable struck many as being incredible, not to say ludicrous.

A moment's use of lead pencil and paper would have shown them, however, that the degree of accuracy required is only (!) about one part in sixty million. The possibility of weighing with this degree of refinement appears to be an accomplished fact.

It is now five or six years that Messrs. Richarz and Krigar-Menzel have been engaged in measuring the diminunition of gravity through a height of some seven feet: a piece of work undertaken at the suggestion of Helmholtz. The experiments were carried out in one of the bastions of the fortress at Spandau. The essential feature of the method is the use of a balance with two pans hung from each arm, these pans being separated by a vertical distance of 226 centimeters. A method of double weighing was employed in which one kilogram, A say, in the *upper left* hand pan was balanced against kilogram B in the *lower right* hand pan. Then kilo A was transferred to the *lower left* hand pan while B is placed in the *upper right* hand pan. Thus the quantity measured is twice the change of weight in either kilo.

In their description of the work, [*Wied. Ann., Bd. 51*, pp. 559-583 (1894)] these gentlemen give a valuable and suggestive discussion of the errors of the balance.

The precautions taken against thermal change, the *bete noir* of physical measurements, were very elaborate. The success with which this was accomplished may be judged from the fact that the presence of a man at the balance case meant changes in temperature from which the balance did not recover sufficiently for the work to proceed, until the lapse of five days (p. 566).

A few hundredths of a degree difference in temperature between the upper and lower pans produced convection currents strong enough to mask the effect under investigation.

Of course the shifting of masses from one pan to the other was accomplished automatically.

Definitive weighings were made only during the late spring and autumn during the changes from the cold to warm season and *vice versa*.

As a final result the loss of weight which a kilo suffers when raised through a distance of 226 centimeters was found to be

$$0.6296 \pm 0.0010 \text{ milligrams.}$$

corresponding to a diminution of gravity amounting to

$$0.0006523 \text{ C. G. S.}$$

The computed change, on the assumption that the Earth is a sphere made up of concentric shells each of uniform density, is

$$0.000697 \text{ C. G. S.}$$

The difference, being in the same direction as that found by Jolly and Thiesen, is ascribed to local variation in the density of the Earth. H. C.

CURRENT CELESTIAL PHENOMENA.

PLANET NOTES FOR JUNE.

Mercury will be "evening star" during June. On the 22d he will be at his greatest distance (elongation) east from the Sun, and will set about an hour and a half later than that body. This month will be a good time both for daylight and evening observations of this planet. Its phase will be gibbous during the first half and crescent during the last half of the month. The moon will pass by Mercury on the evening of June 4, conjunction in right ascension occurring at $10^h\ 32^m$ central time.

Venus will be "morning star" rising about two hours before the Sun. She is getting around toward the farther side of her orbit so that her brightness is decreasing considerably. At the same time her phase is becoming more gibbous. At the beginning of the month 0.67 and at the end 0.76 of her disc will be illuminated.

Considerable has been said lately about the dark part of the disc of Venus being visible, just as the dark part of the new Moon is visible. Several observers claim to have seen the complete outline of Venus' disc a few days before she disappeared in the rays of the Sun this past winter, when her crescent was very narrow. We may say, I think, that this visibility is not from the same cause that renders the dark part of the moon visible, viz.: reflected earthshine. Venus is more than 100 times as far as the Moon from the Earth and therefore would receive less than the ten-thousandth part of the light thrown upon the Moon. The most probable explanation is that Venus has a dense atmosphere, possibly more extensive than that of the Earth, so that her twilight is longer, and extends far enough into the dark hemisphere to become visible from the Earth as a complete ring of light when the crescent of direct illumination is small. The observer discerning the outline of the dark part of the planet, by this faint ring, would naturally have the impression of seeing it all.

Mars will be at quadrature, 90° west from the Sun, June 17, and will be in position to be observed after midnight during this month. Mars will move northeast during June, from Aquarius across a little corner of Pisces into Cetus. The phase of the planet will be smaller this month than at any other time in the year, only 0.84 of the disc being illuminated. Mars will be in conjunction with the Moon, about 3° south of the latter, 48^m after midnight, June 25.

Jupiter and *Neptune* are not to be seen during June.

Saturn is making the turn of the loop in his apparent path among the stars of Virgo. He will begin to move eastward after June 21. The amateur should not fail to make the most of these summer months in the study of this planet. The surface markings on so bright a planet are almost as likely to be seen with a small telescope as with a large one. The Moon will pass by Saturn, 4° south of the latter, June 12, at $2^h\ 41^m$ P. M. central time.

Uranus will be in his most convenient situation for observation during June, being near the meridian during the evening hours. He ought to be easily found by means of stars α and μ Libræ (see Poole Bros. map). Look about 1° 30′ west and 30′ north, *i. e.*, 3 diameters of the Moon west and 1 diameter north, of α, for a star with a dull green disc a little brighter than the star μ.

Planet Tables for June.

[The times given are local time for Northfield. To obtain Standard Times for Places in approximately the same latitude, add the difference between Standard and Local Time if west of the Standard Meridian or subtract if east].

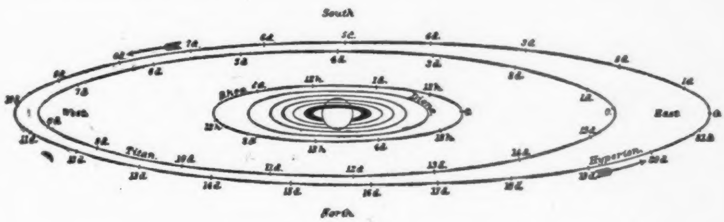
Date. 1894.	R. A. h m	Decl. ° ′	Rises. h m	Transits. h m	Sets. h m
		MERCURY.			
June 5......	6 12.1	+25 31	5 19 A. M.	1 15.3 P. M.	9 11 P. M.
15......	7 20.7	+23 47	5 58 "	1 44.4 "	9 31 "
25......	8 05.7	+20 21	6 21 "	1 50.0 "	9 19 "
		VENUS.			
June 5......	2 04.5	+10 13	2 24 A. M.	9 08.3 A. M.	3 53 P. M.
15......	2 48.2	+13 49	2 13 "	9 12.6 "	4 12 "
25......	3 33.9	+17 01	2 05 "	9 18.8 "	4 32 "
		MARS.			
June 5......	23 20.7	− 6 51	12 49 A. M.	6 25.0 A. M.	12 01 P. M.
15......	23 45.6	− 4 26	12 24 "	6 10.5 "	11 56 A. M.
25......	0 09.6	− 2 03	12 00 "	5 55.2 "	11 50 "
		JUPITER.			
June 5......	4 51.5	+22 03	4 17 A. M.	11 55.0 A. M.	7 33 P. M.
15......	5 01.4	+22 19	3 46 "	11 25.5 "	7 04 "
25......	5 11.2	+22 32	3 16 "	10 56.0 "	6 36 "
		SATURN.			
June 5......	13 12.7	− 4 50	2 30 P. M.	8 14.7 P. M.	1 59 A. M.
15......	13 12.0	− 4 49	1 50 "	7 34.7 "	1 19 "
25......	13 11.9	− 4 51	1 11 "	6 55.3 "	12 40 "
		URANUS.			
June 5......	14 38.8	−15 04	4 38 P. M.	9 40.6 P. M.	2 43 A. M.
15......	14 37.7	−14 59	3 57 "	9 00.1 "	2 03 "
25......	14 36.7	−14 55	3 17 "	8 19.9 "	1 23 "
		NEPTUNE.			
June 5......	4 48.4	+20 58	4 20 A. M.	11 51.9 A. M.	7 24 P. M
15......	4 49.9	+21 01	3 42 "	11 14.2 "	6 57 "
25......	4 51.5	+21 04	3 04 "	10 36.3 "	6 09 "
		THE SUN.			
June 5......	4 54.9	+22 37	4 17 A. M.	11 58.3 A. M.	7 40 P. M.
15......	5 36.3	+23 21	4 15 "	12 00.3 P. M.	7 46 "
25......	6 17.9	+23 23	4 17 "	12 02.4 "	7 48 "
		THE MOON.			
June 2......	3 25.6	+22 32	2 54 A. M.	10 41.0 A. M.	6 45 P. M.
4......	5 41.3	+28 19	4 19 "	12 48.4 P. M.	9 22 "
6......	8 01.7	+25 22	6 41 "	3 00.6 "	11 06 "
8......	10 03.7	+15 22	9 27 "	4 54.5 "	12 05 A. M.
10......	11 47.0	+ 2 31	12 00 M	6 29.6 "	12 45 "
12......	13 22.8	−10 09	2 21 P. M.	7 57.2 "	1 21 "
14......	15 01.3	−20 37	4 40 "	9 27.8 "	2 06 "
16......	16 48.3	−27 04	6 56 "	11 06.5 "	3 13 "
18......	18 40.3	−28 08	8 49 "	12 50.3 A. M.	4 54 "
20......	20 27.6	−23 43	10 05 "	2 28.5 "	6 59 "
22......	22 05.1	−15 04	10 56 "	3 58.9 "	9 12 "
24......	23 36.0	− 3 46	11 32 "	5 21.7 "	11 24 "
27......	1 09.0	+ 8 36	12 07 A. M.	6 46.5 "	1 41 P. M.
29......	2 55.4	+20 13	12 50 "	8 24.8 "	4 16 "

Occultations Visible at Washington.

Date 1894	Star's Name.	Magnitude.	IMMERSION Washington M. T. h m	IMMERSION Angle f'm N p't. °	EMERSION Washington M. T. h m	EMERSION Angle f't N p't. °	Duration. h m
June 15	3 Scorpii..................	7	7 38	150	8 46	267	1 08

Elongations of the Satellites of Saturn.

[In the diagram the points marked 0 are those of eastern elongation of the several satellites. Their positions at intervals of one day after eastern elongation are indicated by the symbols 1*d*, 2*d*, etc].



MIMAS.

		h		
June	2	12.3	A. M.	W
	2	10.9	P. M.	W
	3	9.5	“	W
	4	8.1	“	W
	5	6.7	“	W
	6	4.3	“	W
	7	2.9	“	W
	8	1.5	“	W
	9	1.9	A. M.	E
	10	12.5	“	E
	10	11.1	P. M.	E
	11	9.8	“	E
	12	8.4	“	E
	13	7.0	“	E
	14	5.6	“	E
	15	4.2	“	E
	16	2.8	“	E
	18	12.8	A. M.	W
	18	11.4	P. M.	W
	19	10.0	“	W
	20	8.6	“	W
	21	7.2	“	W
	22	5.8	“	W
	23	4.4	“	W
	24	3.0	“	W
	25	1.6	“	W
	26	11.7	“	E
	27	10.3	“	E
	28	8.9	“	E
	29	7.5	“	E
	30	6.1	“	E

ENCELADUS.

June	1	7.0	P. M.	E
	3	3.9	A. M.	E
	4	12.7	P. M.	E
	5	9.6	“	E
	7	6.5	A. M.	E
	8	3.4	P. M.	E
	10	12.2	A. M.	E

ENCELADUS CONT.

		h		
June	11	9.1	A. M.	E
	12	6.0	P. M.	E
	14	2.9	A. M.	E
	15	11.8	“	E
	16	8.7	P. M.	E
	18	5.5	A. M.	E
	19	2.4	P. M.	E
	20	11.3	“	E
	22	8.2	A. M.	E
	23	5.1	P. M.	E
	25	1.9	A. M.	E
	26	10.8	“	E
	27	7.7	P. M.	E
	29	4.6	A. M.	E
	30	1.5	P. M.	E

TETHYS.

June	2	11.5	P. M.	E
	4	8.8	“	E
	6	6.0	“	E
	8	3.3	“	E
	10	12.6	“	E
	12	9.9	A. M.	E
	14	7.2	“	E
	16	4.5	“	E
	18	1.8	“	E
	19	11.1	P. M.	E
	21	8.4	“	E
	23	5.7	“	E
	25	3.0	“	E
	27	12.3	“	E
	29	9.6	A. M.	E

DIONE.

June	2	12.7	A. M.	E
	4	6.4	P. M.	E
	7	12.1	“	E
	10	5.8	A. M.	E
	12	11.4	P. M.	E

DIONE CONT.

		h		
June	15	5.1	A. M.	E
	18	10.8	A. M.	E
	21	4.5	“	E
	23	10.1	P. M.	E
	26	3.8	“	E
	29	9.5	A. M.	E

RHEA.

June	5	5.9	A. M.	E
	9	6.3	P. M.	E
	14	6.7	A. M.	E
	18	7.0	P. M.	E
	23	7.4	A. M.	E
	27	7.8	P. M.	E

TITAN.

June	3	7.0	P. M.	I
	7	9.9	“	W
	12	12.4	A. M.	S
	15	7.4	P. M.	E
	19	5.1	“	I
	23	8.0	“	W
	27	10.5	“	S

HYPERION.

June	3	5.8	P. M.	E
	10	2.0	A. M.	I
	15	8.7	“	W
	19	6.2	P. M.	S
	24	11.6	“	E
July	1	8.9	A. M.	I

IAPETUS.

May	17	4.3	P. M.	W
June	6	3.7	A. M.	S
	26	4.0	P. M.	E
July	14	5.3	“	I

Elongations of the Satellites of Uranus.

[The diagram shows the apparent paths of the satellites of Uranus during the summer of 1894. The black dots with the numerals indicate the positions of the satellites at intervals of 1 day after each northern elongation. The points marked 0 are those of northern elongation.]

ARIEL.

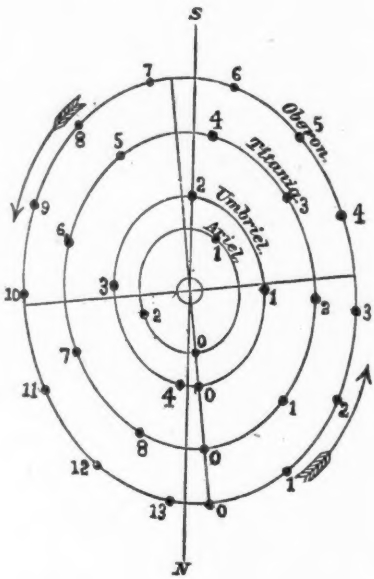
		h	
June	2	5.0 P. M.	N
	5	5.5 A. M.	N
	7	6.0 P. M.	N
	10	6.4 A. M.	N
	12	6.9 P. M.	N
	15	7.4 A. M.	N
	17	7.9 P. M.	N
	20	8.4 A. M.	N
	22	8.9 P. M.	N
	25	9.3 A. M.	N
	27	9.8 P. M.	N
	30	10.3 A. M.	N

UMBRIEL.

		h	
June	1	9.8 P. M.	N
	6	12.4 A. M.	N
	10	3.9 "	N
	14	7.3 "	N
	18	10.8 "	N
	22	2.3 P. M.	N
	26	5.8 "	N
	30	9.2 "	N

TITANIA.

		h	
June	2	7.3 A. M.	N
	6	3.8 P. M.	S
	11	12.2 A. M.	N
	15	8.7 "	S
	19	5.2 P. M.	N
	24	1.5 A. M.	S
	28	10.2 "	N



OBERON.

		h	
June	3	8.3 A. M.	S
	10	1.9 "	N
	16	7.6 P. M.	S

OBERON CONT.

		h	
June	23	1.2 P. M.	N
	30	6.8 A. M.	S

Maxima and Minima of Variable Stars.

[From ephemerides by Dr. Loewy in the "Companion to the Observatory," and by Dr. Hartwig in the "Vierteljahrsschrift der Astronomische Geselischaft".]

MAXIMA

June	1	R Leonis
	1	W Hydræ
	2	S Aquarii
	2	V Virginis
	3	V Coronæ
	3	R Cygni
	5	R Scuti
	6	R Aquilæ
	7	R Sagittæ
	8	S Boötis
	12	Z Cygni
	13	U Herculis
	17	V Cancri
	17	U Monocerotis

MAXIMA

June	19	R Lyræ
	19	S Camelopardi
	19	S Libræ
	22	R Lyncis
	23	X Boötis
	23	V Boötis
	24	R Cassiopeiæ
	25	R T Cygni
	26	S Pegasi
	26	V Libræ
	29	Z Libræ
	30	U Geminorum

MINIMA

June	4	R Virginis
	4	R Lyræ
	6	R Vulpeculæ
	6	V Cephei
	9	R Draconis
	13	X Libræ
	13	W Herculis
	14	U Piscium
	15	X Cygni
	20	T Aquarii
	21	S Geminorum
	22	X Ophiuchi
	23	R Hydræ
	29	S Vulpeculæ

Minima of Variable Stars of the Algol Type.

[Given to the nearest hour in Central Standard Time.]

U CEPHEI.

		h
June	2	12 M.
	4	12 midn.
	8	12 M.
	10	12 midn.
	13	11 A. M.
	15	11 P. M.
	18	11 A. M.
	20	11 P. M.
	23	11 A. M.
	25	11 P. M.
	28	10 A. M.
	30	10 P. M.

S CANCRI.

June	9	10 A. M.
	18	9 P. M.
	28	9 A. M.

δ LIBRÆ.

June	2	8 A. M.
	4	3 P. M.
	6	11 "
	9	7 A. M.
	11	3 P. M.
	13	11 "
	16	7 A. M.
	18	2 P. M.
	20	10 "
	23	6 A. M.
	25	2 P. M.
	27	10 "
	30	6 A. M.

U CORONÆ.

June	1	7 P. M.
	5	6 A. M.
	8	5 P. M.
	12	4 A. M.
	15	3 P. M.
	19	2 A. M.
	22	12 M.
	25	11 P. M.
	29	10 A. M.

U OPHIUCHI.

June	1	12 M.
	2	8 A. M.

U OPHIUCHI Cont.

		h
June	3	4 A. M.
	3	12 midn.
	4	8 P. M.
	5	4 "
	6	12 M.
	7	8 A. M.
	8	5 "
	9	1 "
	9	9 P. M.
	10	5 "
	11	1 "
	12	9 A. M.
	13	5 "
	14	1 "
	14	10 P. M.
	15	6 "
	16	2 "
	17	10 A. M.
	18	6 "
	19	2 "
	19	10 P. M.
	20	7 "
	21	3 "
	22	11 A. M.
	23	7 "
	24	3 "
	24	11 P. M.
	25	7 "
	26	4 "
	27	12 M.
	28	8 A. M.
	29	4 "
	29	12 midn.
	30	8 P. M.

Y CYGNI.

June	1	12 midn.
	3	12 M.
	4	12 midn.
	6	11 A. M.
	7	11 P. M.
	9	11 A. M.
	10	11 P. M.
	12	11 A. M.
	13	11 P. M.

Y CYGNI Cont.

		h
June	15	11 A. M.
	16	11 P. M.
	18	11 A. M.
	19	11 P. M.
	21	11 A. M.
	22	11 P. M.
	24	11 A. M.
	25	11 P. M.
	27	11 A. M.
	28	11 P. M.
	30	11 A. M.

S ANTLIÆ.

(Every third minimum.)

June	1	11 A. M.
	2	10 "
	3	9 "
	4	9 "
	5	8 "
	6	7 "
	7	7 "
	8	6 "
	9	5 "
	10	5 "
	11	4 "
	12	3 "
	13	3 "
	14	2 "
	15	1 "
	16	1 "
	17	12 midn.
	17	11 P. M.
	18	11 "
	19	10 "
	20	9 "
	21	9 "
	22	8 "
	23	7 "
	24	7 "
	25	6 "
	26	5 "
	27	5 "
	28	4 "
	29	3 "
	30	3 "

Phases and Aspects of the Moon.

		Central Time. d	h m
New Moon	June	3	4 56 P. M.
Perigee	"	4	11 40 P. M.
First Quarter	"	10	7 14 A. M.
Full Moon	"	18	1 06 A. M.
Apogee	"	20	4 50 A. M.
Last Quarter	"	26	4 03 A. M.

COMET NOTES.

Discovery of a New Comet (*a* 1894, Denning).—A telegram from Mr. John Ritchie, Jr., Boston, March 28, announced the discovery of a faint comet by Mr. W. F. Denning of Bristol, England, the following being the discovery position:

March 26.396 Gr. M. T. R. A. $9^h\ 55^m$. Decl. $+ 32° 15'$.

The comet was observed at Northfield on the evening of March 28, and found to be a very small object, very difficult to see in the 5-inch finder, but easily seen and measured with the 16-inch telescope. It had a well defined nucleus of the 11th magnitude, with nebulosity surrounding it between 1′ and 2′ in diameter. It had a short, slightly spreading tail, 2′ or 3′ long. From our own observations on the dates March 28, April 1, and April 5, we have computed the following parabolic elements of the comet's orbit:

$$\begin{array}{rll} & \text{Time of perihelion} & = 1894 \text{ Feb. } 14.1900 \text{ Greenwich mean time.} \\ \pi = & \text{Longitude of perihelion} & = 133° \ 26' \ 15'' \\ \Omega = & \text{Longitude of node} & = \ \ 75 \ \ 34 \ \ 12 \quad \Big\} \text{ Mean equinox } 1894.0 \\ i = & \text{Inclination} & = \ \ \ 6 \ \ 31 \ \ 06 \\ q = & \text{Perihelion distance} & = 1.22497. \end{array}$$

These elements do not represent the middle place, the observation of April 1, with a sufficient degree of accuracy, the outstanding residuals being $+ 8''$ in longitude and $+ 26''$ in latitude. These residuals cannot be reduced on any assumption of a parabolic orbit, and it may be presumed that the orbit will turn out to be an ellipse of comparatively short period.

As the comet is growing rapidly fainter it is not probable that those using small telescopes will see it. We therefore give the following ephemeris only to indicate approximately the course of the comet and its increasing distance from us. Its path among the stars during April and May, is shown upon Poole Bros' Map in this number.

EPHEMERIS OF COMET *a* 1894.

	R. A.	Decl.	log Δ	log r	Br.
	h m s	° ′			
Mar. 28	10 02 24	+ 31 01	9.7080	0.1421	1.00
Apr. 5	10 28 36	26 44	9.7632	0.1605	0.71
13	10 50 16	22 44	9.8201	0.1799	0.50
21	11 08 28	19 05	9.8778	0.1999	0.35
29	11 24 36	15 47	9.9345	0.2201	0.25
May 7	11 39 08	12 48	9.9895	0.2403	0.17
15	11 52 24	10 07	0.0428	0.2601	0.12
23	12 05 04	7 40	0.0933	0.2797	0.09
31	12 16 08	+ 5 33	0.1385	0.2965	0.07

A New Comet Discovered in Australia.—A telegram from Mr. John Ritchie April 6 announced the discovery of a comet by Mr. Gale at Sydney, Australia. The discovery position was:

April 2.944 Gr. M. T. R. A. $2^h\ 30^m\ 48^s$; Decl. $- 55° 35'$.

The motion is easterly. The comet is described as round with a bright condensation. This comet is too far south to be visible at any of the northern Observatories.

A Comet's Tail Discovered by Holmes.—Another telegram received April 11, announces the discovery of a bright comet's tail by Holmes April 9. Its approximate R. A. and Decl. were $17^h\ 58^m$ and $71° + 30'$.

Ephemeris of Tempel's Second Periodic Comet (1873 II)—Mr. L. Schulhof gives an ephemeris of this comet, in *Astronomische Nachrichten*, No. 3219, for the month May 19 to June 16. The elements used are as follows:

Epoch and osculation: 1894, April 25.0 Paris mean time.

$$\left.\begin{array}{l} M = \;\; 0^\circ \;\; 15' \;\; 27'' \\ \pi = 306 \;\; 14 \;\; 22 \\ \Omega = 121 \;\; 10 \;\; 02 \\ i = \;\; 12 \;\; 44 \;\; 20 \end{array}\right\} 1894.0 \qquad \begin{array}{l} \varphi = 36^\circ\, 26'\, 34'' \\ \mu = 679''.860 \\ \log a = 0.478392 \end{array}$$

At the time of the last observation of this comet in 1878 its brightness was somewhat less than that which it should be theoretically during this month. It is therefore to be hoped that the comet will be found at this apparition.

EPHEMERIS.

		R. A. h	m	s	Decl. °	′	log Δ	$1 : r^2\Delta^2$
May	19	0	18	39	− 2	48.6	0.22119	0.190
	20		21	43	2	37.5		
	21		24	46	2	26.6	0.22049	
	22		27	48	2	15.7		
	23		30	50	2	04.9	0.21982	0.188
	24		33	50	1	54.2		
	25		36	50	1	43.6	0.21917	
	26		39	49	1	33.1		
	27		42	47	1	22.7	0.21853	0.186
	28		45	44	1	12.4		
	29		48	40	1	02.3	0.21791	
	30		51	35	0	52.3		
	31		54	30	0	42.3	0.21730	0.184
June	1	0	57	24	0	32.5		
	2	1	00	16	0	22.8	0.21670	
	3	1	03	07	0	13.3		
	4		05	58	− 0	03.9	0.21611	0.182
	5		08	48	+ 0	05.4		
	6		11	37	0	14.5	0.21552	
	7		14	25	0	23.5		
	8		17	11	0	32.3	0.21492	0.179
	9		19	56	0	41.0		
	10		22	41	0	49.6	0.21432	
	11		25	25	0	58.0		
	12		28	08	1	06.3	0.21371	0.177
	13		30	50	1	14.4		
	14		33	31	1	22.4	0.21309	
	15		36	11	1	30.2		
	16	1	38	49	+ 1	37.9	0.21246	0.174

Mr. Schulhof says that the uncertainty of the time of perihelion passage cannot be more than ± 2 days. Should this occur two days early the R. A. of the comet would be increased $3^m\ 35^s$ May 15 and $2^m\ 55^s$ June 16, and the declination would be increased 10′ May 15 and 11′ June 16. If the perihelion passage should be 2 days later than calculated the R. A. would be decreased $3^m\ 43^s$ May 15 and and $3^m\ 02^s$ June 16, and the Decl. decreased 10′ May 15 and 11′ June 16. The observer searching for the comet may need to sweep over a space 20′ wide, extending 1° each way, east and west, from the predicted place of the comet.

Comet *b* **1894 (Gale).**—From Science Observer Circular No. 105, we take the following:—A later message received April 15, contained the elements as given below, which were computed by Kreutz. From these an ephemeris has been computed by the Rev. G. M. Searle, which is given below.

ELEMENTS.

$T = 1894$ April 13.82 Greenwich M. T.
$\left.\begin{array}{l} \omega = 324^\circ\ 18' \\ \Omega = 206\ \ \ 9 \\ i = 87\ \ \ 24 \end{array}\right\}$ Mean Eq. 1894.0
$q = .9856$

EPHEMERIS FOR GREENWICH MIDNIGHT.

	R. A.			Decl.		Light.
	h	m	s	°	′	
April 28	7	14	40	− 27	35	5.47
May 2	8	5	28	− 11	36	
6	8	47	32	+ 2	44	
10	9	21	0	+ 15	15	3.21

Light, April 2 = 1.

The object announced as a new, bright comet, by Holmes, in the position, R. A. $17^h\ 58^m$, Decl. $+71^\circ\ 30'$, proves not to have been a comet.

The comet discovered by Denning on March 26 was observed here the next night, March 27, 8 hours 75 Meridian time, in approximate position, R. A. $9^h\ 58^m + 31^\circ\ 29'$. It was an easy object in the 10-inch refractor, having a very small but sharply defined nucleus, so close to the edge of the nebulosity that it was somewhat doubtful at first sight whether it belonged to the comet or was a star. A few minutes watching showed that it was a part of the comet. From the stellar nucleus extended a short, broad fan-shaped tail. Many fruitless searches were made here for the bright comet reported to be discovered by Holmes, of England on April 9. As neither direction or rate of motion were given in the announcement, a large region of the sky in the place indicated was swept over until it became evident that some mistake had been made by Mr. Holmes, and this, I have just learned, was really the case. This experience emphasises once more the importance of *always ascertaining motion* beyond the possibility of a doubt before making a public announcement. If this is not possible at discovery, then the suspected comet should be telegraphed to some leading observatory, preferably to Harvard by American observers, with instruction to with hold the public announcement until verified by the observation of motion, either at the observatory so notified, or by the discoverer himself. This simple precaution would often save the very considerable expenditure of a world wide announcement, and what is often of far greater importance, the waste of many hours of valuable time. To all of us, at times, a *clear night* is above riches.

WILLIAM R. BROOKS.

Smith Observatory, Geneva, N. Y., April 18, 1894.

Photograph of the Pleiades.—Plate No. XI gives a reproduction of the central part of the photograph referred to on page 192, March No. of ASTRONOMY AND ASTRO-PHYSICS.

The finer details of the nebula and many of the fainter stars are necessarily lost in reproduction by means of the screen, but if the reader will hold the picture about two feet from the eye, the meshes of the screen will be lost and the effect of the photograph in a measure realized.

One of the two asteroid trails, that of (203) Pompeja, is shown near the right edge of the plate about three fourths of an inch from the bottom. The other trail, of 1894 AW, was on the upper part of the plate which has been cut off in reproduction.

NEWS AND NOTES.

In No. 110 of the Johns Hopkins University circulars will be found an address by Maurice Bloomfield on "A century of comparative Philology." In that address it is claimed that a Hindu by the name of Tilak has very recently made important discoveries concerning early date, of Vedic literature. It is also stated that Tilak's discovery is based upon astronomical data recorded in the Vedic literature.

Nautical Office Investigation.—In Vol. XII, pp. 664 and 760 of this publication, some notice was given of the investigation of the Nautical Office at Washington, D. C., while the same was in progress. Inasmuch as this was a public investigation, it was expected that some public notice of the findings of the court of inquiry would be given that persons interested might know the result. Upon inquiry after the report of the investigation had been sent to the Secretary of the Navy, it was learned that the results of the investigation were not for publication. We then wrote directly to the Secretary of the Navy for information concerning the results for publication, and the reply substantially was, that there was no matter for publication such as was asked. This was nearly three months after the report was in his hands and yet one of the parties in the investigation claimed he did not know what the decision in the case was. It may be that he had not used the proper means to ascertain the result, but one would suppose that he would do so from self-interest, and he claimed that he had sought this information from reasonable sources without success. Writing to Professor Newcomb later about the case he kindly placed in our hands a copy of the Secretary's letter, which, by our request, he permits us to publish. It throws some light on the results reached. It fully exonerates Professor Newcomb. It is as follows:

(COPY).

NAVY DEPARTMENT, WASHINGTON, Nov. 3, 1893.

PROFESSOR SIMON NEWCOMB,

Superintendent Nautical Almanac Office, Washington, D. C.

SIR:—I have carefully examined for myself the report made by Captain McNair, the memoranda submitted by the Judge Advocate General of the Navy, and the testimony taken in the matter of the charges preferred against you by Dr. J. R. Morrison, and take pleasure in saying to you that there is no evidence whatever to show that you were guilty of the charge preferred by Dr. Morrison of improperly using employees under you to do private work for you during office hours. The evidence exonerates you from the charge.

I think, however, that it appears from the testimony in this case that it would be of advantage to the Nautical Almanac to have the benefit of your continuous superintendence of the work of that office and that it would be better to revoke the leave of absence heretofore granted to you to attend the Johns Hopkins University as a lecturer two days in the week, durings its sessions. The department does not, however, impute to you any blame for such absence. You were permitted to assume the duties of a professor at the Johns Hopkins University with full knowledge on the part of the Department that it would necessitate your absence from your duties as Superintendent of the Nautical Almanac, and the order which has been issued revoking such leave to you from and after the 1st.

of January next, is simply carrying out the policy which has been recently applied in other cases, and which was determined upon after full and careful consideration.

Taking great pleasure in thus exonerating you from all personal blame in this matter, I am,

Very respectfully yours,
H. A. HERBERT,
Secretary of the Navy.

This investigation has affected both parties to it seriously, and we can not imagine why it has been deemed best to withhold from the public the results of this public investigation.

The United States Coast Survey Office.—Under date of March 12, the New York *Evening Post* contains a column devoted to the present "peril" of the United States Coast Survey office, as now constituted and managed. The blow at it is said to be aimed through a provision in the sundry civil appropriation bill which provides for a transfer of the Coast Survey office from the Treasury department to the Navy department. The writer of the article speaks in complimentary way of its present chief officer, Professor T. C. Mendenhall who is well and favorably known, and so far he is right. Whether his grave charges of political scheming against those who are now seeking a change are true or not, we do not know. However this may be, the writer before referred to gives an interesting bit of history regarding the changes in administration at the Survey office. We extract the following:—

"The first law establishing the survey was passed in 1807, and the Treasury department was given custody of it by President Jefferson. The interruptions caused by the war of 1812 with Great Britain were followed by a transfer of jurisdiction to the Navy department in 1818, and this connection continued till 1832, when Congress passed a law enlarging somewhat the scope of the survey, which went back to its old place under the Treasury. This change was undoubtedly largely influenced by the strong recommendation of Samuel L. Southard, who, as secretary of the Navy in 1828, repeatedly declared that the work had not progressed as it ought under his department, and that the charts prepared were disconnected and untrustworthy, and the operations as a whole "very expensive in proportion to their usefulness."

Hardly had the new order been in force two years, when President Jackson caused the transfer of the survey once more to the Navy department, of which Levi Woodbury was then secretary. For two years this experiment lasted. Mr Woodbury meantime had taken the Treasury portfolio; and by 1836 both the President and Mr. Woodbury had become so well satisfied of their error that they restored the survey to the Treasury, where it remained undisturbed till 1843. Then Congress passed a law in connection with the annual appropriations, requiring that the President should appoint a board consisting of civilians, naval officers, and army engineers, submit the question of the organization of the survey to them, and organize it on the plan approved by a majority of the board. This board, though composed of only three civilians and six commissioned officers of the army and navy, recommended that the survey "should be under the control and considered a part of the Treasury department." This recommendation was adopted, and was generally accepted as satisfactory till 1848, when another attempt was made in Congress to transfer the survey to the Navy department, but was promptly quashed by a vote in the house of 90 against, to 36 in favor of

the scheme, and in the Senate, some months later, when the advocates of the transfer could muster only 2 votes. In 1851, the project having been revived, it was given its death blow by a report of secretary Corwin of the Treasury, answering a Senate resolution of inquiry, in which he reviewed the subject historically and convinced the doubtful members of Congress that it would be best to let things stand as they were.

During the civil war the services rendered by the civilian survey were repeatedly recognized, but most strikingly by Commodore D. B. Porter, who not only tells how he was furnished with accurate charts on which to plan his water campaigns in the South, but describes in one of his letters his dependence on the personal presence of two of the civilian members of the survey who were helping him on board of his flagship, declaring: "I cannot speak too highly of these gentlemen. I assure you that I shall never undertake a bombardment unless I have them at my side."

Another attempt was made to dislodge the survey in 1884, but was given up because the joint commission from both houses of Congress that looked into the matter satisfied themselves that no change would be of benefit to public interests. The reasons against a transfer are even stronger now, since the rehabilitation of our navy has provided employment for so many more officers educated for naval service. It is extremely difficult to-day for the navy to spare the officers actually needed in the survey work under its existing organization; only about half the number formerly detailed to the survey are available at present."

Meridian Circle Work at Washington.—In a letter dated March 26, 1894, Mr. A. N. Skinner first assistant astronomer of the U. S. Naval Observatory, states that he is actively engaged in the observation of the "*Astronomische Gesellschaft*" zone — 13° 50′ to — 18° 10′. He has made since he commenced, some two months since, more than 2,000 observations. The zone contains 8,689 stars, each of which must be observed twice, once in each position of the instrument. He is assisted by Messrs. Littell and King.

The New York Academy of Sciences. Stated Meeting, March 26th, 1894.—The meeting organized with President J. K. Rees in the chair.

A paper by O. F. and A. C. Cook, entitled "A Monograph of Scyonotus," was read by title and referred to the Publication Committee.

The Section of Astronomy and Physics then organized with Professor Rees as Chairman and Professor Hallock as Secretary *pro tempore*.

In the absence of Mr. Jacoby his paper on 61 Cygni was read by Professor Rees. This paper has been published recently in full in the *Monthly Notices* (No. 2 Vol. LIV., Dec., 1893) of the Royal Astronomical Society of London. Professor Rees called attention to the importance of all observations showing changes in the relative positions of the components of 61 Cygni. He remarked on the present uncertainty as to whether the pair formed a true binary system or not. Calculations had given such differing orbits as are indicated by periods of 1159 and 462 years. The observations of S. W. Burnham lead him to conclude that the members of the pair are separating, and Professor Hall, from his observations extending over a period of 12 years, favors the view of a physical connection of these stars (Astr. Journal, No. 258, page 140).

Professor Hallock read a paper on a method of defining standard colors, and showed many samples of colors and the five discs of standard color used. The methods of defining standard color with them were illustrated.

Professor Rees remarked upon the importance of the work.

Dr. See, of the University of Chicago, then discussed the "Origin of the Heavenly Bodies," illustrated with lantern slides. He believes that the double stars originated by the swinging apart of one nebula through a process of splitting.

The paper was discussed by President Rees and others.

Publications of the Lick Observatory, Vol. II, 1894.—Double Star measures made with the 36-inch and the 12-inch Clark Refractor of the Lick Observatory in 1889, 1890 and 1891, by S. W. Burnham.

This publication is an interesting volume and prepared in excellent way. The micrometrical measures of the double stars are given individually accompanied by important notes from various sources pertaining to many of them and illustrations of special features of some give added value to the data. The large, clear, page and the excellent printing are in keeping with the character of the work and make the volume a very desirable one for the astronomer's library.

The frontispiece is a fine photogravure plate of the micrometer of the 36-inch refractor. The kinds of work done by Mr. Burnham and published in this catalogue may be given under the following heads:—

Measures of stars noted as double in Krueger's Catalogue of the Astronomical zone 55° to 65°.

Zusatz by Professor Krueger.

Measures of stars noted as double in the *Astronomische Gesellschaft*, zone 65° to 70°.

New Nebulæ.

Measures of Planetary Nebulæ. Observations of Nebulæ with the 36-inch refractor of the Lick Observatory.

New Double stars discovered at the Lick Observatory in the years 1888 to 1892.

Fourteenth Catalogue of new double stars discovered at the Lick Observatory.

Fifteenth Catalogue of new double stars discovered with the 36-inch refractor of the Lick Observatory.

Sixteenth Catalogue of new double stars discovered at the Lick Observatory in May, June, July, 1889.

Seventeenth Catalogue of new double stars discovered in 1890 with the 36-inchequatorial of the Lick Observatory.

Eighteenth and Nineteenth Catalogues of new double stars. Additional notes.

List of the Catalogues of new double stars discovered by S. W. Burnham.

Works issued by the Lick Observatory.

The additional notes cover eighteen pages and form a very useful part of the publication because the matter is to date and the illustrations of orbits are numerous and deduced by modern approved methods.

Chandler's Criticisms of the Harvard Photometric Observations.—Last month we called attention briefly to criticisms made by Dr. Chandler in *A. N.* 3214, in regard to the Harvard photometric observations. We then thought and still do that his criticisms are not without bias. This sentence or two from the first paragraph is a sample. He says: "To be more specific, a list of some of these contradictions is here given. It is by no means complete, but comprises perhaps the more important results of a somewhat desultory examination, which was carried far enough to justify, as it seemed to me, an impression of distrust whether any of these observations are suitable for any precise or critical purpose."

His "desultory examination" is sufficient to justify a distrust in "any of these observations" . . . "for any precise or critical purpose. Is this fair and friendly criticism? We do not think it is at all "legitimate" criticism, if by that word is meant fair and friendly dealing with the work of another. We do not question Dr. Chandler's facts, but we do condemn his broad inferences.

The Peters-Borst Star Catalogue.—About 1889, the manuscript of a catalogue of 35,000 stars was completed at Litchfield Observatory of Hamilton College and made ready for publication. The observations and reductions were made in the main by Charles A. Borst, then assistant to Dr. C. H. F. Peters, director of Litchfield Observatory, who, it was claimed, planned the work and directed it in the beginning. When the manuscript was completed Mr. Borst refused to give it up, claiming it as his own. The matter was taken to court and tried before Judge Williams in Utica in the spring of 1889, and it was decided that the catalogue belonged to Dr. Peters. This trial attracted wide attention in consequence of the scientific features in the case and the prominence of the witnesses called to testify. Since the death of Dr. Peters which occurred in 1890, the contest has been maintained by Hon. Elihu Root of New York as administrator in the appeal made by Mr. Borst to the Court of Appeals of New York. From the Utica *Herald*, April 14, we learn that the Court of Appeals hand down a decision reversing that rendered in 1889 by Judge Williams and ordering a new trial.

While neither of the parties claimed that the star catalogue had commercial value, it was shown by the testimony of Professors Hall of Washington, and Boss of Albany, that the cost of work was not less than $12,000. For a statement of the case in important particulars, see Vol. VIII *Sidereal Messenger*, pp. 138, 455.

A Brilliant Meteor.—On April 10th at 8:35 P. M. a very brilliant meteor of exquisite beauty, moving possibly at the rate of 10 or 12 miles a second, spanned an arc of the heavens east to north. It entered onr atmosphere between the stars ε and ζ Herculis, sailing in the direction of γ Ursæ Minoris. It disappeared near the star θ Draconis. Its duration did not exceed 7 seconds of time. Its color was of a brilliant cerulean blue, followed by a short trail, orange and purple in appearance. Its shape was elliptical, the major axis in the line of motion seemed to be fully 40 minutes of an arc, somewhat larger than the diameter of the Moon, whereas the minor axis did not exceed 15 minutes. These figures may be somewhat exaggerated; as a brilliant object projected suddenly on a dark background by an optical illusion, appears invariably much larger than its real dimensions. This meteor did dot explode, which shows that it did not belong to the family of bolides. The star-gazers of Belize, who were happy enough to catch a glance of this large meteor must have realized that they were the witnessers of a very rare and beautiful phenomenon.

C. M. CHARROPPIN, S. J.

Corozal, British Hinduras, Central America, April 18, 1894.

While observing recently with the 6-inch Grubb equatorial of this Observatory, the writer picked up a double star in the constellation of Canis Major, which appears to be new. In the hope that it may prove a new binary system, the writer measured the star as carefully as circumstances would allow on two nights with our Grubb position micrometer. Owing to inexperience in this sort of work, the faintness of the star in so small a telescope, and the poorness of the driving clock, these measurements leave much to desire.

March 28, 1894 — 5″.18 — 277°.5
March 31, 1894 — 5 .25 — 276 .0
Power = 150.

The magnitues are, possibly, 9.5 and 9.4.

The well-known double star, Lalande 14292, precedes this star by 46 seconds, and is 10′ south of it. The position of Lalande 14292 for 1880, as taken from Vol. 4 of the Publications of the Cincinnati Observatory, is:

$$\text{R. A.} = \quad 17^h\ 14^m\ 12^s$$
$$\delta = -21°\ 50'$$

This gives the new double an R. A. of $7^h\ 14^m\ 58^s$ and δ of $-21°\ 50'$ for the same year.

ROGER SPRAGUE.

Chamberlain Observatory,
Denver, April 2, 1894.

From Canada.—Mr. John A. Paterson, M. A., Vice-President, presided over the meeting of March 20, 1894, the Astronomical and Physical Society of Toronto. Interesting letters were read from M. Paul Henry, of the National Observatory, Paris, France; Mr. E. W. Maunder, F. R. A. S., of Greenwich Observatory; Professor W. H. Pickering of Arequipa, Peru; Professor S. W. Burnham, of Chicago; Professor W. W. Payne, Editor of ASTRONOMY AND ASTRO-PHYSICS; Lieutenant A. G. Winterhalter, U. S. N.; Mr. Rudkins, Secretary of The Mathematical and Physical Society of the University of Toronto and others, several of whom wrote in terms of high praise of the last annual Report just issued by the Astronomical Society.

A report upon the subject of using contracted apertures, by stopping down telescopes by inserting a card-board disc, covering the central part of the object glass, was sent in by Dr. J. C. Donaldson, of Fergus, who had found that the defining power of a first-class instrument did not suffer.

The Chairman read a series of extracts from a somewhat remarkable article by Sir Robert Ball, recently printed in *The Fortnightly Review*, entitled "Significance of Carbon in the universe."

Several instructive and entertaining papers on Magnetism were read by Mr. G. G. Pursey, Mr. J. A. Collins and Mr. Thomas Lindsay. Experiments were performed by Mr. Collins; also by Mr. A. Aronsberg, who brought with him a powerful magneto-machine.

Meeting of April 17: Chair occupied by Mr. John A. Paterson, M. A., Vice-President.

Communications included letters from Miss Agnes Clerke, of London and from the Royal Society of England asking the Society to consider the advisability of assisting in establishing a central office, or bureau, to be sustained by international contributions, and to be charged with the duty of compiling a complete catalogue containing titles of scientific publications, whether appearing in periodicals or independently, the titles to be arranged not only according to authors' names, but also according to subject matter. If sufficient support be given, the new catalogue will commence with the year 1900. Through Mr. Charles Carpmael, F. R. A. S., an appeal was presented from the University of Cambridge, addressed to friends of the University and to all interested in Astronomical Science, for assistance to raise $11,000 to complete the celestial photographing equipment of the University Observatory.

Under observations, Mr. Thomas Lindsay read the first report of work intended to be done with the Wilson telescope; Mr. G. G. Pursey handed in a number of drawings of the solar disc, with spots, obtained very satisfactorily by prejection, and Mr. J. A. Copland contributed detailed description of a recent aurora.

Consideration of "magnetism" was resumed, a series of short papers being

read. Mr. Lindsay, was introductory in character, and in a popular style. Mr. W. Barlour Musson's dealt chiefly with the experiments conducted many years ago and related by Baron Reichenbach. Mr. A. Elvin's, was illustrated by a number of unique and clever experiments, at which Mr. J. R. Collins assisted.

To finish, there was an address by Mr. A. J. McDonald, inventor of The McDonald Tellurian, an extremely simple and inexpensive device for explaining astronomical motions.

The Chicago Academy of Sciences. *Section of Mathematics and Astronomy, April 9th.*—The regular monthly meeting was held at the Chicago Athenaeum; Professor G. W. Hough, President, in the chair. After the transaction of routine business, the Section proceeded to the program of the evening. Mr. A. C. Behr read the first paper on "*Observations of the Pleiades since the time of Ptolemy.*" The speaker discussed the observations of Hipparchus and Ptolemy as given in Baily's edition of the catalogue, and referred to the probable variability of η Tauri. Ulugh Beigh's rating of this star as 4th magnitude appears verified by a comparison of 31 3d magnitude and 54 4th magnitude stars in the catalogues of Hevelius, Halley and Tycho. A change of nearly two magnitudes in other stars of the group probably formed the basis of R. Wolf's opinion that some of the brighter stars are slowly variable. A chart by Mr. Peck, F. R. A. S., "The Pleiades as seen by Ptolemy, A. D., 137" was exhibited on the screen; seven stars are shown, 27 and 28 Tauri not included. Pickering's explanation of the absence of 28 Tauri by its bright line spectrum, would leave 27 Tauri still unaccounted for; while according to the legend it was 17 in the extreme opposite portion of the group which disappeared. Mr. Behr did not regard the chart as trustworthy. He referred to the bright line C in the spectrum of η Tauri recently discovered by Campbell, and also to Miss Maury's notes on the spectrum of 28 Tauri. He said that the drawings of Wolf, M. Hall, Temple, von Littrow, and the photographic plates of Common, Roberts and Henry show no nebulosity near 27 and 28 Tauri; but nebulosity had been observed by Spitaler and others at Vienna. In regard to Jeauriat's observation in 1768, Ranyard had said "doubtful, no nebula drawn;" Jeauriat had only marked the Bessel stars 31 and 32 'nebuleuse."

This seemed to be worthy of attention, since 31 and 32 and the visible nebulosity are in the same region. Mr. Behr thought that Bessel's measures of the Pleiades and those recently made by Elkin would in time disclose the mechanism of the cluster.

A general discussion followed the reading of the paper. Professor Burnham did not think some of the observations mentioned were very trustworthy, but made some interesting remarks on Barnard's nebula near Merope, which he declared was a most difficult object to observe. Professor Hough made some remarks on the distribution of the nebulous matter in the Pleiades, and on the nebulosity of the heavens generally. Dr. T. J. J. See read the second paper of the evening, entitled "*A Visit to Some Eastern Observatories,*" which gave a sketch of the work he found in progress at the Observatories which he had recently visited while making a journey to Washington, Baltimore, New York and Boston. He reported that astronomical work was making good progress in the East, and said his journey had been a most agreeable one. After some further discussion the Section adjourned.

T. J. J. SEE,
Recorder.

Errata.—Page 311, line — 2, for 10940 read 20940. Page 312, line + 24, for These read Three. The last one is comparatively unimportant, the first one is. Possibly they were not plain in the MS.

PUBLISHER'S NOTICES.

The subscription price to ASTRONOMY AND ASTRO-PHYSICS in the United States and Canada is $4.00 per year in advance. For foreign countries it is £1 or 20.50 marks per year, in advance. Recent increase in price to foreign subscribers is due to increase of postage because of enlarged size during the year 1892. Messrs. Wesley & Son, 28 Essex Street, Strand, London, are authorized to receive subscriptions. Payment should be made in postal notes or orders or bank drafts,

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For information of correspondents, the names and addresses of the associate editors of ASTRO-PHYSICS are given as follows:—

James E. Keeler, Observatory, Allegheny, Pa.; Henry Crew, Northwestern University, Evanston, Ill.; Jos. S. Ames, Johns HopkinsUniversity, Baltimore, Md.

All matter or correspondence relating to General Astronomy, remittances, subscriptions and advertising should be sent to Wm. W. Payne, Publisher and Proprietor of ASTRONOMY AND ASTRO-PHYSICS, Goodsell Observatory of Carleton College, Northfield, Minn.; and the Associate Editors for General Astronomy are: S. W. Burnham, Government Building, Chicago Ill.; E. E. Barnard, Lick Observatory, Mt. Hamilton, Cal., and H. C. Wilson, Goodsell Observatory, Northfield, Minn.

Manuscript for publication should be written on one side of the paper only and *special care should be taken to write proper names and all foreign names plainly*. All drawings for publication should be *smoothly* and *carefully* made, in *India Ink* with lettering well done, because such figures are copied exactly by the process of engraving now used. If drawings are made about double the size intended for the printed page, better effect will be secured in engraving than if the copy is less in size. It is requested that manuscript in French or German be typewritten. If requested by the authors when articles are sent for publication, *twenty-five* reprint copies, in covers, will be furnished free of charge. A greater number of reprints of articles can be had if desired, at reasonable rates.

Rates for advertising and rates to news agents can be had on application to the publisher of this magazine.

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